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ORIGINAL ARTICLES.

I.—Note on a nearly perfect Skeleton of *Ichthyosaurus tenuirostris* from the Lower Lias of Street, Somerset.

By R. Lydekker, B.A., F.G.S., F.Z.S., etc.

(PLATE IX.)

Some ten years ago Mr. Alfred Gillett observed in a quarry of the Lower Lias near his residence at Overleigh, Street, Somerset, a number of broken slabs of shaly limestone, containing portions of the skeleton of an Ichthyosaur. These slabs, which had been cast aside by the workmen, were fitted together by Mr. Gillett, who finally succeeded, after the expenditure of great pains, in skilfully developing from them an almost entire example of the skeleton of *Ichthyosaurus tenuirostris*, Conybeare.

With his usual liberality Mr. Gillett presented this remarkably beautiful specimen to the Geological Department of the British Museum (Natural History) in 1884, where it is now exhibited on the east wall of Gallery No. XI, forming one of the most striking objects in the fine series of Reptilian remains from the English Lias.[1]

The gift of this specimen to the Museum was particularly opportune, since the collection, although rich in some of the other species of the genus, was previously but poorly supplied with examples of *Ichthyosaurus tenuirostris*; and is even now widely distanced in this respect hy the Dublin Museum of Science and Art. Mr. Gillett's specimen, of which we give a figure in Plate IX., although by no means a large example of the species, is one of the best preserved skeletons that have ever come under our notice. The animal lies on its ventral surface, with the back exposed and the limbs symmetrically expanded on either side; and it is largely due to the symmetrical position in which it has been preserved that the beauty and apparently unusual perfection of the specimen are due. Almost the only damage that the skeleton has sustained consists of some crushing of the skull, and the loss of a few of the tail vertebræ, and of some of the small bones of the paddles.

The fortunate rescue of this interesting specimen by Mr. Gillett affords a good instance of the value of local observers in preserving rare fossils which would be otherwise totally lost.

We may remind our readers that the species before us is not a typical representative of the genus *Ichthyosaurus*, of which the

[1] The specimen is referred to in the Museum Catalogue of Fossil Reptiles, pt. 2, p. 84, No. R. 498 (1889).

Ichthyosaurus tenuirostris, Conybeare.
From the Lower Lias of Street, Somerset.
Presented to the British Museum (Nat. Hist.) by Alfred GILLETT, Esq.

normal forms are *I. communis* and *I. intermedius*. In those species the front paddles are characterized by their great width, and the large number of longitudinal rows of ossicles entering into their composition. In the group represented by *I. tenuirostris*, on the other hand, the paddles are much narrower and longer, and have fewer rows of ossicles. Thus, as is well shown in our figure, the third digit, or the one taking origin from the intermedium of the carpus, consists of only a single row of ossicles; and there are usually only four rows of these ossicles in the entire limb, against seven or eight in the typical groups. Moreover, the radius—the bone lying immediately below the humerus on the front side of the limb—is a nearly square bone, with a notch on its front border; whereas in the typical group it is much wider and longer, and has no such notch.

All these characters indicate that the group represented by *I. tenuirostris* is much less specialized than the typical one, and thereby less widely differentiated from more ordinary reptiles. Indeed, so different is the structure of the paddles in *I. tenuirostris* and *I. communis*, that if we had these alone to deal with there could be but little hesitation in regarding the former as the representative of a distinct genus. Unfortunately (or perhaps our purely geological readers would prefer to say fortunately), however, there are certain intermediate forms which render it much more difficult to define such genera; so that for the present at least our species may rest in the genus to which it was referred by its original describer.

The specific characters of the present species are to be found in its long and slender rostrum, narrow fluted teeth, and the presence of a notch on the hinder border of the coracoid. Specimens in the British Museum with the rostrum still more elongated than ordinary were separated by Sir R. Owen as *I. longirostris*, but these proved to be identical with the earlier *I. latifrons* of König. The magnificent series of specimens from the Lower Lias of Barrow-on-Soar in the Dublin Museum indicates, however, that these variations are not of specific value, so that all these forms should apparently be referred to *I. tenuirostris*.

We may conclude this notice by calling attention to a point where the British Museum Catalogue is in error. It appears from the recent researches of Dr. E. Fraas that the type of *I. acutirostris*, Owen, has smooth carinated teeth, like those of *I. platyodon*, so that this species should be transferred to the Platyodont group, which it has been proposed to raise to generic rank as *Temnodontosaurus*. This leaves the name *I. quadriscissus* as the one best applicable to the other specimens catalogued as *I. acutirostris*. Moreover, Dr. Fraas considers that *I. zetlandicus*, Seeley, is identical with *quadriscissus*; and we are disposed to doubt the right of separating *I. longirostris*, Jäger (*non* Owen) from the same. Finally, we observe with satisfaction that Dr. Fraas is disposed to consider the American *Baptanodon* as inseparable from *Ophthalmosaurus* of the English Oxford and Kimeridge Clays, of which such a fine series has recently been acquired by the British Museum.

II.—The Perched Blocks of Norber Brow and their Levels relative to their Place of Origin.

By T. Mellard Reade, C.E., F.G.S., F.R.I.B.A.

IN a recent excursion the Liverpool Geological Society visited Norber Brow, near Austwick, to inspect the celebrated perched blocks of Silurian rock lying upon the Carboniferous limestone plateau. The visit was made in very appropriate weather during a storm of hail which added a weird element to the scene and heightened by contrast the blackness of the Silurian blocks. Since returning home I have re-read Prof. McKenny Hughes' interesting paper on the subject,[1] and find that generally speaking my notes and measurements are in accord with his. The angularity of the perched blocks, so different to the rounded and striated erratics of the Boulder Clay Plains of Lancashire and Cheshire, and the absence of Boulder Clay, is very striking, and inevitably suggests their transportal by glacier ice probably at the last phase of the glacial period.

It has, I believe, been generally taken for granted that these blocks are higher than their origin, and have been, as stated by Prof. Hughes, pushed up hill from the north.[2] Being specially interested in this question, which was raised on the spot by our excellent guide Dr. Ricketts, four of our party, after descending into the valley, climbed up the western side and traversed it northwards towards Crummack. The first object that attracted attention was a magnificent glaciated surface or *roche moutonnée* of Silurian rock on our left ascending the slope to the west transversely to the direction of the valley. On the right at about the level of the perched block on Norber Brow figured in Prof. Hughes's paper (p. 530), which I shall refer to as Perched Block No. 1, and which we took for our datum level, was what may be fitly described as a mason's yard of black angular Silurian blocks, as large or larger than those on Norber Brow. The bed rock was Silurian also and further northward up the valley appeared to be much glaciated. Continuing our ascent we reached the junction of the Silurian with the Carboniferous Limestone, and following the contour northward came upon a jutting crag of Silurian from which several huge blocks had separated at the joint planes a distance of a foot or two. These blocks had a slight cant down hill, and were just in a condition and position for transportal had the valley been filled with glacier ice. The upper surface of the crag appeared to be rounded. The rock evidently breaks up naturally into cuboidal masses. Here then was probably the origin of the blocks of Norber Brow, as according to the aneroid observations made by me and checked by Mr. Ashton Hill, C.E., this Crag is 265 feet above Perched Block No. 1 on Norber Brow. The highest point of the Silurians we tried by aneroid was 295 feet above the same datum. It is evident that the movement of the blocks towards Norber Brow was in accordance

[1] On some Perched Blocks and Associated Phenomena, Q.J.G.S. vol. xlii. pp. 527–538 (1886).

[2] Q.J.G.S. vol. xlii. pp. 531 and 538.

with the laws of gravity and downhill. A glacier filling the Crummack Valley would move southward and the blocks in question clinging to the west side of the valley might be stranded where they are found on Norber Brow. There is no doubt that the pedestals and platforms on which most of them stand, and which vary from one to two feet in height, are due to the wasting away of the surrounding limestone rock as pointed out by Prof. Hughes and Mr. Mackintosh, but I quite agree with Prof. Hughes that more data than are at present available would be required before any reliable estimate of the time that has elapsed since the close of the Glacial Period could be worked out.

These remarks are not intended to apply to the Silurian blocks above Settle, which Phillips states are 200 feet above any similar rocks in the district *in situ*.[1] As we did not see them or test their levels, any remarks of mine would be superfluous.

III.—The Lower Cretaceous Series of the Vale of Wardour.

By A. J. Jukes-Browne, F.G.S., and Rev. W. R. Andrews, F.G.S.

IT has long been known that certain deposits of Lower Cretaceous age lay between the Gault and the Purbeck group in the Vale of Wardour, but the absence of any good open sections, along the tracts where they reach the surface, has hitherto prevented geologists from ascertaining the exact nature and succession of the beds.

Dr. Fitton, whose account of the Vale of Wardour is wonderfully good and accurate, distinctly recognized the existence both of Wealden and Vectian (Lower Greensand), stating that certain sands, containing traces of marine shells, occurred beneath the Gault and above the clays which he regarded as Wealden.

Mr. Bristow, however, when surveying the district in 1851 to 1853, does not seem to have obtained any evidence for the separation of the sands, and considered it safer to colour all the sands and clays below the Gault as Wealden until clear evidence of their marine origin could be obtained. Hence no Lower Greensand is indicated on the Geological Survey Map.

During last year we jointly resurveyed a portion of the ground on the six inch maps, and were able to obtain evidence that proved Fitton's view to be correct, and goes far towards completing our knowledge of the succession in this district.

This preliminary notice is printed by permission of the Director-General of the Geological Survey. Early in 1890 a well was sunk at Dinton, which gave us important information regarding the beds immediately beneath the Gault. The following is an abstract account of the section thus obtained.

		Feet.
Gault	Yellow, brown, and blue clay (with fossils)	21½
	Sandy rock with a layer of small pebbles at the base (fossils)	14½
Vectian	Brown, grey, and yellow sands, with lumps and layers of ferruginous sandstone	26½
	Light grey sandy clay, becoming darker and passing down into stiff black clay	7
		69½

[1] Rivers and Mountains of Yorkshire, 1855, p. 111.

One of the ferruginous layers in the sands has yielded a fairly good specimen of *Exogyra sinuata*, which is one of the most characteristic fossils of the Vectian group.

In this well the base of the group is evidently not reached, but most fortunately it is completed by a brook section at Teffont, which begins in a black clay exactly like that found at the bottom of the well. This black clay is about six feet thick, and passes down into a nearly black sand which has a green streak when cut, and consists mainly of dark-green grains of glauconite.

Underneath this sand are mottled clays, which were recognized by Mr. Whitaker as similar to the "catsbrain" clays of the Weald,—their tints are yellow and white, mottled here and there with a rich claret-coloured stain, which imparts a special character to the clay. Below are yellow loamy clays. We consider these to be of Wealden age, and the dark sand to be the base of the Vectian; but as the section is not clear, and this sand has not yet been found elsewhere, we cannot say whether the sand is conformable to the clays or not.

The maximum thickness of the Vectian appears to be about forty feet, and the yellowish-green sands which underlie the pebbly base of the Gault can be traced westward for several miles both along the southern and the northern sides of the Vale, overlapping the Wealden and the several divisions of the Purbeck beds, and gradually thinning out.

The thickness of the Wealden clays is difficult to estimate, because exposures are so few and the slopes are so gentle; but there can hardly be more than 40 or 45 feet at the east end of the vale, and they thin out rapidly towards the west, not extending far beyond Teffont on the north side, nor far beyond Sutton Row on the south side.

Eastward the Wealden occupies a certain width of flat ground which merges into the alluvium east of Dinton Station. At the cottages north of the station a well was sunk for about forty feet through yellow-grey and brown clays. We were informed that there was a perfect succession of clays without any sudden change or any pebbly bed, but that at the bottom there was *a floor of very hard sandstone*. The accuracy of this information we proved by having the well emptied, and the bottom tested with a punch. A piece of this sandstone was given to one of us by the well-sinker, and is a hard grey calcareous grit with *Cypridea punctata*. The material thrown out of the well also yielded *Cyprides*, *Paludina carinifera*, and *Unios*. We believe these clays to be of Upper Purbeck age, for the lower part of them, with a band of calcareous grit, exactly like that in the well, can be seen in the first cutting west of the station, and they are succeeded by a set of clays, marls, and thin limestones, which have a much greater resemblance to the Purbeck beds than to the Wealden.

When the railway was made there must have been an excellent section of these beds and the upper part of the Middle Purbecks in the two cuttings west of Dinton Station; now they are so over-

grown that it is very difficult to make out the relations of the strata, for they are certainly flexured and perhaps faulted. We feel sure, however, that the beds in the first cutting are higher than those in the second, and we hope in a future paper to publish the evidence on which we base our conclusions. If our reading is correct, there is a well-developed Upper Purbeck series in the Vale of Wardour, with a thickness of 70 or 80 feet; and this is succeeded by representatives of the Wealden and Vectian series, which, however, are poorly developed and taken together are less than 100 feet thick.

IV.—The Recent and Rapid Elevation of the Himalayas.

By Henry H. Howorth, Esq., M.P., etc.

IN the last Number of the Geological Magazine my friend Mr. W. T. Blanford takes exception in very courteous terms to the views I have maintained in regard to the recent elevation of the Highlands of Eastern Asia. I cannot, however, quite grasp how far he agrees or disagrees with me. He does not apparently question the evidence which has so impressed experienced observers like Humboldt, Murchison, Tschihatcheff, Cotta, and Senkofski in regard to the absence of traces of widespread glacial phenomena in the high mountains of the Ural, the Altai, and the Thian Shan ranges, and which apart from all considerations seems to me inexplicable on any other theory than that these mountains did not exist during the so-called Glacial Period.

Mr. Blanford limits his criticism to the Indian evidence; but even here I do not quite understand what is the exact position he maintains. In the Manual of Indian Geology, that splendid encyclopædia of facts on whose title-page his name occurs with that of Mr. Medlicott, it is stated over and over again that traces of glaciation are nowhere present in peninsular India. In the same "Manual" it is argued that the so-called glacial phenomena which have been said to occur in the Punjaub and other parts of the Plains are to be otherwise explained, and I believe I am right in attributing to Mr. Blanford the view that the old glaciers of the Himalayas never protruded into the plains.

This limits the problem therefore to the Himalayas themselves. Mr. Blanford objects to Mr. Campbell's very emphatic evidence because he had never penetrated into the inner valleys, and only spoke of the outskirts where the valleys open out. This is true; nor does Mr. Campbell claim to have done more than deduce his conclusions from what he saw. He was a very keen observer, he had seen glacial phenomena in all parts of the world, he went specially to study glacial phenomena on the spot, and his descriptions of what he saw, which are very minute and graphic, seem to me to be most conclusive that the lower parts of the Himalayan valleys show no traces of glaciation.

But I did not quote Mr. Campbell only. General McMahon, who has studied and written much on the Himalayas, and published his observations in the Records of the Indian Geological Survey, is just

as emphatic as Mr. Campbell. Lastly, it is true I have not myself been there, but I have seen many photographs, and many drawings, and notably the magnificent drawings of the Himalayan scenery by Colonel Trotter, which show the contour and structure of the valleys so admirably. These were exhibited only a few days ago at the Geographical Society, and I was struck, as other people were, by the angular, sharp, crisp outlines of the rocks and the absence of the peculiar curves that denote glacier action, and this not merely on the watersheds, but in the valleys themselves. The drawings, be it remarked, represented nearly the whole length of the chain.

I must not be misunderstood. I did not say in my paper that there are not traces of the glaciers having once extended further. I said very plainly indeed that there are such traces. Sir J. Hooker and Mr. Blanford proved this long ago. The Himalaya glaciers seem to me like glaciers in other parts to have shrunk considerably. What I do maintain is that these traces are only found when we have mounted the valleys several thousand feet, and that they are quite incommensurate in size and importance with the vast glacial *débris* and phenomena which should be found on the flanks and in the neighbourhood of these gigantic mountain buttresses if during the so-called glacial age they had formed the feature in the landscape which they do now. As gathering ground for ice at a time when Central Asia was occupied by water instead of being an arid plain, they are incomparably great, when contrasted with the Dovre Fjelds which shed their *débris* as far as the Carpathians, Central Russia, and Norfolk; or the Alps whose *débris* occurs down the Rhone valley as far as Lyons, and if they had existed in the so-called Glacial age we ought to find the great Indian plain strewn widely with unmistakable *débris*, instead of having to painfully search for it until we reach a height of 7000 or 8000 feet. I must, therefore, claim Mr. Blanford as supporting my conclusion rather than criticizing it. In regard to the main question, an important witness is Mr. Blanford himself. Let me quote testimony for which he was responsible when he was in the very thick of his geological labours in India, and long before he had largely forsaken the happy hunting grounds of Geology for those of Zoology.

On page lvi of the introduction to the Manual of Indian Geology, in discussing the origin of the Himalayas, we read, "The whole of the gigantic forces, to which the contortion and folding of the Himalayas and the other extra peninsular mountains are due, must have been exercised in the interval which has elapsed since Eocene times. . . . The direction of the Himalayan ranges is clearly due to post-Eocene disturbance. It will be shown, in the chapters relating to sub-Himalayan rocks, that the movement has been distributed over the Tertiary and post-Tertiary period; *and a great portion of it is of post-Pliocene date.*" Again, "As it is certain that a great portion of the disturbances affecting the Himalayan strata are of Pliocene or post-Pliocene date, it is reasonable to conclude that *at the close of the Miocene epoch no such mountain barrier as exists at present separated the Indian peninsula from Central Asia.* There is

independent evidence in favour of the view that the elevation of the Thibetan plateau is of post-Siwalik date; for remains of *Rhinoceros* and other large mammals occur at an elevation of 15,000 feet in Thibet, and it is not probable that these animals lived in so elevated a region" (*id.* pp. 585 and 586).

Here is assuredly an important witness in support of the views of Falconer and of Strachey, behind whose broad ægis I claim to stand.

In conclusion, there are two statements in Mr. Blanford's communication which I wish to traverse. He says Mr. Lydekker is of opinion that the Rhinoceros remains from the Hioondes plateau are of Pliocene age. Mr. Lydekker was once of that opinion, but he subsequently withdrew it, and has printed his opinion that he now holds the beds in which they occur to be of post-Tertiary age.

In another venture Mr. Blanford says, "That if I had had an opportunity of seeing what is perhaps the grandest example of subaerial denudation in the world, I should be as little inclined as he is to believe that such gigantic furrows as the Himalayan valleys can have been ploughed out by rain and rivers since the frozen Mammoths were imbedded in the Siberian tundras."

My answer is that I do not for a moment believe that these furrows were ploughed out by rain and rivers at all, and that it seems to me as incredible to believe that the splintered precipitous and sharp-edged precipices of the Himalayan valleys were carved out by such agencies as that the Matterhorn or the pinnacles of the Sierra Nevada or other similar objects were thus shaped. The theory which attributes such forms to this kind of denuding agency seems to me absolutely transcendental, and to involve a *reductio ad absurdum* of Uniformitarian Geology.

V.—On Dynamic Metamorphism.[1]

By the Rev. A. Irving, B.A. and D.Sc. (Lond.), F.G.S.

In the originally epistolary form of my previous communication[2] on this subject brevity was aimed at as far as possible. It was written at the time the letters therein referred to appeared, and my object in throwing out such suggestions as I have ventured to make, was to call the attention of writers on petrological subjects to the desirability of avoiding a certain looseness of thought, which seemed to attach itself not infrequently to the terms 'chemical change' and 'chemical action.'

Mr. Fisher's letter[3] in reply thereto is in reality an appeal from the creed of the geologist, not to *chemistry*, but to the creed of the chemist. I am certain it will satisfy no one who has made a real study of chemical physics.

The instances of the formation of CO and NO which have been cited, along with a good many of the immense number of "parallel

[1] Written in January last.

[2] See Geol. Mag. Dec. III. Vol. VII. pp. 562–564.

[3] See the letters of Mr. Fisher and Mr. Harker in the Geological Magazine for January, 1891.

instances" (*loc. cit.*), are very well known to students of chemistry; but they are not to the point. There is the error which mistakes the algebraic sum of the results of a chemical reaction (as expressed in thermal units) for pure chemical combination, *quâ* entry into atomic union. I thought that 'chemical combination' was a phrase sufficiently guarded, if taken in its technical and literal sense; and I still think so. All that Mr. Fisher's 'authority' has urged, and a good deal more, was before my mind when I penned the sentence: "It is something much more complex than that" (p. 563).[1] Mr. Fisher's authority thus takes a different ground altogether from mine, when he enunciates the doctrine that "many chemical changes are attended with the disappearance of heat." Of course they are. Why, every case of dissociation (which is a 'chemical change') is attended with the disappearance of heat or of an equivalent amount of electrical energy (in electrolysis). But all such cases will at once be seen to be rigidly excluded from the term "chemical combination." The fact is the term chemical change, as illustrated by the authority who uses it, covers the whole process of the reaction in each case; it includes the antecedent dissociation (or partial dissociation), bringing on what I have elsewhere called the "quasi-nascent state,"[2] which must precede the recombination of the atoms or radicles to form new molecules. It is of the latter only that I speak in my December letter; and it must be quite obvious that pressure is antagonistic to the former. We may put it thus:—If $\theta =$ the amount of kinetic energy manifested (*quâ* heat) as the result of a reaction of two bodies on one another; further, if we take $x =$ the amount of heat used up in the preliminary dissociation (or partial dissociation) of the antecedent molecules; and if $y =$ the amount of heat evolved in the recombination of the atoms thus rendered available; the very simple equation

$$\theta = y - x$$

seems to me to cover the facts of the case, in the simplest form in which the problem can be presented to us; as (*e.g.*) in the combination of two volumes of hydrogen and one volume of oxygen to produce steam. And if, as in a comparatively small number of cases, there follows a secondary dissociation into proximate constituents different from those which formed the antecedents of the reaction, calling the heat expended in this x', we get the equation

$$\theta = y - (x + x').$$

All that Mr. Fisher's authority has contributed then to the discussion is to inform us that in the cases he has cited (and there are plenty of others) the value of x (or in some cases of $x + x'$) exceeds the value of y, in which case θ is negative, and the phenomenon is *endothermic*. On the other hand, if the value of y

[1] Those who wish for real information on the subject may be referred to Pattison, Muir's "Thermal Chemistry" (1885), and the English edition of Ostwald's "Outlines of General Chemistry" (1890), both published by Macmillan.

[2] See Chemical News, Nos. 1402 and 1505, "On Dissociation and Contact Action."

exceeds that of x (or $x + x'$), θ is positive, and the phenomenon is *exothermic*.[1] The subsequent, or even simultaneous absorption of heat by a diluent (such as nitrogen, in all cases of bodies burning in air) makes no difference, for this belongs to the *distribution* of the heat, and makes no difference in the *quantity of heat generated*, which is the same for the same weight of the same materials concerned in the same reaction.[2]

We know, for instance, that heat is used up (endothermic) in the reaction $C + CO_2 = 2CO$, for the simple reason that more heat is required to effect the dissociation $CO_2 = CO + O$, than is evolved in the subsequent combination (molecule for molecule) expressed by the equation $C + O = CO$; and we are pretty sure that this is the true explanation of the observed phenomena, because we know from independent evidence that the heat of combustion of C into CO is less than one-half of that of the combustion of the same amount of carbon in the reaction $CO + O = CO_2$. These facts have found their way for years past into such well-known text-books of chemistry as Williamson's "Chemistry for Students" (see § 60, 2nd ed.).

Again, in the case of NO we require the high temperature of the electric spark-discharge to effect the antecedent dissociation of the N_2 and O_2 molecules before we get NO formed; but in this case I am not at all sure that the phenomenon is endothermic, though the rapid distribution of the heat of combination may deceive us, if we lose sight of the all-important distinction between quantity and intensity of heat (absolute temperature).[3] The absence of explosive phenomena proves nothing as to this point. All this and a good deal more of like import has been discussed by myself and others in published papers during recent years.

The question raised practically resolves itself into this: Does pressure promote the dissociation of previously stable molecules, so as to render the intra-molecular energy of their atoms available for entry into new combinations? When this is answered in the affirmative on the strength of cogent evidence, I will gladly admit the 'storage of energy' (*quâ* potential energy of chemical affinity) in the crust of the Earth as the result of mere pressure. The latest grand conception of Mendeléeff as to the nature of chemical affinity points however exactly in the opposite direction.

In the cases of change of volume to which Mr. Fisher's authority refers, I suppose diminution of volume is meant. But I am not sure that the very molecular strain involved in surface-tension may not in these cases initiate chemical action; and a suspicion remains that the gases of the supernatant atmosphere may have something to do with it. I am not, however, acquainted with the details of the experiments referred to, nor do I think that for purposes of dynamical geology they are much to the point.

[1] For a clear exposition of these principles see a paper by Spencer U. Pickering, F.R.S., on "Chemical Action and the Conservation of Energy," *Nature*, vol. xliii. pp. 165–167.

[2] Very well illustrated in the case of ammonia, which will burn continuously in pure oxygen, but not in common air.

[3] See my "Chem. and Phys. Studies, etc." p. 54.

It is stated that, "Whether it be atomic energy or not, is not at present known." Does this mean that we cannot distinguish between the energy of translation of the molecules of a gas or the energy of cohesion of the molecules of a solid, and the intra-molecular energy which the atoms possess *per se*? Is it meant to say that recent advances in chemical physics have not led to the recognition of the atom-temperature within the molecule as affecting for the time being the stability of the molecule itself? Does it mean to tell us that we cannot, in the light of a host of known facts, draw a distinction between those molecular moods or states of matter which we call magnetic and electric, and that interaction between the atoms which is concerned in building up those molecules, and which we call chemical affinity? Or, turning to his own chosen examples, can Mr. Fisher's authority bring forward any array of cogent facts to shake or overthrow the doctrine that the avidity with which CO takes up oxygen (whether free or combined) when the temperature is raised, and that most characteristic avidity with which NO takes up O_2 (at ordinary temperatures) is in both cases due, not to any force resident in the molecule as such, but to the *unsaturated valency* of the C-atom or the N-atom respectively? Intra-molecular energy seems to me as clearly distinguishable from molecular energy, as the latter is from "ordinary mechanical or molar energy."[1]

Reverting now to Mr. Fisher's original paper in the Geological Magazine (July, 1890), I think I can follow his reasoning pretty clearly, until on p. 304 he says: "We must now inquire into what forms of energy the work has been converted." Here there seems to me some confusion of thought. 'Energy' is the capacity for doing work; that is, for producing or retarding motion, or overcoming resistance to motion. The amount of energy expended is always proportionate to the work done. In the case before us the energy is presented in the mechanical form of pressure, and if the lateral pressure is the result of the original 'energy of position' of another portion of the Earth's crust, the actual source of the energy is *gravitation* acting upon the mass favourably situated. This being in part resolved into lateral thrust, the energy of that thrust is expended in doing work which is distributed among the following terms:[2]—

1. The lifting of the cover (potential energy of position);
2. The elevation of the centre of gravity of the deformed mass, in so far as the extension of the mass takes place upwards (potential energy of position);
3. The work of compression (molecular friction generating heat);
4. The work of *shearing*, *bending*, and *fracture*.

Where I am unable to follow Mr. Fisher's reasoning is with reference to the last term, which is surely *outside* the others altogether.

[1] Since writing this paper I have noticed that the above cases with some others are discussed in a very suggestive (though scarcely exhaustive) manner by Professor Liveing in his little book "Chemical Equilibrium, the Result of Dissipation of Energy" (1885).

[2] Referring to the geometrical construction used by Mr. Fisher (*loc. cit.*).

All that he considers the result of shearing seems to me the direct result of the lateral pressure; shearing, bending, and fracture being simply *concomitant items of expenditure* of the energy of the lateral pressure. Shearing, bending, and fracture are simply work done in opposition to the force of *cohesion*; the force, that is to say, which tends to maintain the molecules of a solid at fixed distances and in fixed relative positions, to which the undeformed mass owed such rigidity as it possessed; and this important factor Mr. Fisher seems to have overlooked. In both shearing and bending we must also have some molecular friction, with its thermal effect; but it is not easy to see how even this applies in the case of *fracture*, which is simply the work of overcoming cohesion. When Mr. Fisher says "this part of the *energy* is not convertible, etc.," he seems to me to be using the word 'energy' where he ought to use the term 'work.' Herein appears to lie the fallacy of the whole argument; and through this, as through a loop-hole, the idea of chemical action seems to insinuate itself.[1]

Turning now to Mr. Harker's letter, the first thing that calls for remark is the "*direct correlation of mechanical work and chemical energy.*" This is very nicely put; so nicely that the unwary reader is easily led off the scent. On this I simply ask for an explanation of the word 'correlation.' What lies hid behind that rather big word is the gist of the whole question we are trying to solve. Sorby certainly *suggested* something of the sort; but we must have something more solid than suggestions, as a basis upon which to construct sound scientific theory. If Dr. Sorby or any one else will show how we can step by step follow such a transformation in harmony with what some of us *know* from years of thoughtful and fairly extensive study at first-hand of the facts and phenomena which form the groundwork of chemical theory, some real advance will be made. Until this is done, we shall have to regard the cited suggestion as no more than a gigantic guess. It may be useful in pointing the way to further investigation; but to draw deductions from it, as if it were a well-established induction, is altogether unscientific.

Mr. Harker refers, however, to experiments of Cailletet, Pfaff, and Spring, as affording practical verification of the suggestion of Dr. Sorby. Unfortunately this rough massing together of experimental evidence gives us a result of such a neutral character as we are familiar with in ordinary laboratory-processes of alkalimetry. I know some of Cailletet's splendid work very well, but this has hardly any bearing upon the question before us. If Mr. Harker refers to his experiments on the effect of pressure upon the interaction of zinc and sulphuric acid, or to those of Pfaff upon the

[1] An illustration will perhaps make this clear. We may expend a definite amount of energy furnished either by the muscles of a horse or the fuel consumed in the fireplace of an engine in drawing a series of loaded trucks along a *perfectly horizontal* line of rails. Work is done in overcoming the friction of the wheels against their axles and against the rails, and in the displacement of a portion of the atmosphere with the movement of the train; but would any one contend that energy was stored up in the train?

interaction of hydrochloric acid and calcspar, and upon the combination of water with dehydrated calcium sulphate, they all tell against the notion of pressure helping chemical action. Pfaff draws this general conclusion from them: "It follows quite decisively from these experiments, that high pressure *intercepts* chemical affinity, when for its activity an increase of volume is necessary."[1] All this seems to have been clear enough to Mr. Harker some six years ago, when he wrote:[2] "The effect of increased pressure is to facilitate such physical and chemical changes as involve contraction of volume in the substances acted upon, and to retard changes which are accompanied by expansion." If he will furnish references to later work of Pfaff in which his conclusion cited above is superseded, he will confer a favour upon all who are interested in this fundamental question of petrology.

As to the work of Dr. W. Spring, assuming that the alleged crystalline copper sulphide was proved to be a definitely crystallized chemical compound, we have to consider whether such alleged combination is accompanied by increase of density. We may attack the question I think fairly in this way:—

Assuming the following constants:—

	At. Wt.	Sp. Gr.
Metallic copper	Cu = 63·5	8·9
Elementary sulphur ...	S = 32	2·07

and dividing each at. wt. by the sp. gr., multiplied into the molecular weight of water, we have for our present purpose,

$$63{\cdot}5 \div (8{\cdot}9 \times 18) = {\cdot}39 \text{ (vol. ratio of Cu atom):}$$
$$32 \div (2{\cdot}07 \times 18) = {\cdot}86 \text{ (vol. ratio of S atom).}$$

If the combination were merely 'additive,' we should have ·39+·86 (= 1·25) for the vol. ratio of the fundamental Cu S molecule.

But the sp. gr. of Cu S (crystalline) being 4·6, we get

$$(63{\cdot}5 + 32) \div (4{\cdot}6 \times 18) = 1{\cdot}14$$

for the actual vol. ratio of the same molecule. This tells us plainly enough that the union of atoms is accompanied by an *increase of density*.

Mr. Harker is careful to point out the necessity for keeping the apparatus cool in the experimental work of Dr. Spring, as cited by him. Where, one may ask, would be the necessity for this, if (i.) there were no heat generated by friction during compression; (ii.) the process of chemical combination (as alleged) were endothermic? We have no right even here to say that any portion of the mechanical energy is transformed into chemical energy. The chemical energy was potentially already in the atoms of Cu and S respectively, as a function of their atomic weights (according to one of the latest generalizations of chemical science); all that the pressure did in the experiment in question was to coerce them

[1] See *Allgem. und Chem. Geol.* pp. 308–310: "Es geht also aus diesen Versuchen ganz entschieden hervor, dass starker Druck die chemische Verwandtschaft dann *aufhebt*, wenn zu der Entfaltung ihrer Wirksamkeit eine Volumvermehrung esforderlich ist."

[2] Brit. Assoc. Report, Aberdeen Meeting (1885), p. 846.

into such intimate contact as to bring them *within the field of its operation*.[1] This, too, seems to be Spring's own interpretation of the results obtained.[2]

A case, the exact reverse of that of Cu S, has been established by the more recent work of Dr. Spring in conjunction with Van 't Hoff.[3] The substance acted upon was the double acetate of copper and lime. The formula for this double acetate is $Cu(C_2H_3O_2)_2+Ca(C_2H_3O_2)_2+8H_2O$, and it occurs as 8-sided prisms of the quadratic system. It is gradually built up with the aid of moderate external heat by adding gradually free acetic acid to a warmed mixture of equivalent proportions of neutral acetate of copper and slaked lime in water.[4] The process is a complicated one; but it is not reversible, for in the experiment referred to the double acetate is resolved by a pressure of 7000 atmospheres into the two separate acetates of copper and lime with the separation-out of some of the water of constitution, the same change in fact as is effected by heating the salt above 75° C. at ordinary atmospheric pressure. The effect of pressure here is again to effect a transformation which is accompanied by loss of volume; tending to give greater closeness to the atoms until they obtain that configuration which belongs to their most stable relation. We may compare it with the action of heat in breaking up the molecules of ammonium nitrate to such an extent as to allow the atoms to rearrange themselves (with evolution of heat) in the more stable compounds N_2O and H_2O. The fact is one which, taken along with the case of Cu S, is of far-reaching significance, but cannot be followed up further here. So far as it goes, it lends support to a 'diagenetic' rather than to an 'epigenetic' theory of metamorphism.[5]

Mr. Harker misrepresents me, when he suggests that I have assumed that "the whole of the work done in the compression, deformation, and friction of rocks passes into heat." I have never assumed anything so nonsensical, as Mr. Harker may see, if he will do me the favour of making a more intimate acquaintance with my little work, to which he refers. Neither he nor I was dealing with a case of 'deformation,' but with a hypothetical case of what Heim calls 'überlastet,' the condition, that is to say, of 'latent plasticity' antecedently to deformation, where the burden upon the deep-seated rock-mass produces hydrostatic pressure greater than the internal resistance to deformation which the cohesion can offer. All that

[1] This was pointed out by me in App. ii. note D of my original thesis written in 1887 (see "Chem. and Phys. Studies," etc., p. 108).

[2] See "Am. Journ. Sci." vol. xxxv. (1888) p. 78, and vol. xxxvi. (1888) p. 285, *et seq.* I am much obliged to General MacMahon for furnishing me with these references.

[3] See "Zeitschrift für physik. Chemie," i. 5, cited in 'Nature,' vol. xxxvi. p. 160.

[4] Wislicenus, "Organische Chemie," pp. 541, 542.

[5] From certain known facts, it does not seem at all improbable that such complex syntheses may have taken place extensively in the early stages of the genesis of rock-forming minerals. Among them I have suggested ("Chem. and Phys. Studies," etc., App. ii. note Q) the formation of minerals of the spinel type, in which Al_2O_3 plays the part of an *acid*, the alumina being set free with subsequent alteration of physical conditions, to enter into new relations as a *base*.

I have maintained is that during the compression-stage you must have molecular friction (or something equivalent thereto), just as, while crushing is going on, you must have molar friction (if the phrase may be allowed); and in both cases you must get *some* of the energy expended manifested kinetically as heat. My real contention was, that the *intensity* of such heat would be too low for it to be of any practical importance.

To sum up, I must confess that in the light of what has been adduced in this paper, I see no reason for unsaying or qualifying anything in my previous letter[1] (December, 1890), though I am very sorry if any one's susceptibilities have been wounded. It was quite unnecessary for Mr. Harker to offer me any apology; but I am a little surprised that he should not have read what was put into print as a criticism or stricture upon his own most valuable essay, which was printed in the Report of the Brit. Assoc. in 1885.

The one great factor of mineral change at depths is *superheated water*,[2] the existence of which is dependent on pressure. This both Mr. Fisher and Mr. Harker seem to have overlooked. But the experiments of Daubrée and others go to show that its action is in many cases in the direction of the resolution of higher and more complex compounds into simpler and denser mineral forms, rather than in the building-up of more complex out of simpler compounds.

[Since this paper was written I have received from M. Troutschoff of St. Petersburg, a copy of his interesting paper in the "Comptes Rendus," in which he gives an account of his method of preparing crystalline quartz from a dialysed solution of silica in water by prolonged heating in hermetically-sealed tubes; and in "Nature," vol. xliii. p. 545, the somewhat startling announcement appears of his successful achievement of the *synthesis of hornblende*, by a similar method, out of the various oxides which generally enter into the composition of that mineral. These were obtained partly as dialysed solutions, partly as fresh-precipitated hydrates, and mixed in right proportions as such. Each tube before sealing up was exhausted by a Sprengel pump, so that probably most of the uncombined water was drawn away as vapour along with the air. The sealed tubes were heated in a specially-constructed sand-bath for three months to a temperature of 550° C. Under these conditions the synthesis of hornblende crystals was effected, together with some pyroxene, a zeolite, and a variety of orthoclase; and it is not difficult to see that here synthesis of the denser minerals was facilitated by the action under great hydrostatic pressure of the superheated water which remained in the tubes, and separated out from the hydrated materials as the digestion proceeded. M. Troutschoff applied in fact the conditions which may easily be conceived as having obtained generally in the genesis of the heavier minerals of the Archæan schists. See my work (*Ibid*, pp. 68, footnote, and 91, 95, 96).]

[1] With one exception, which no one has noticed. When I said 'shear-planes' I should have said 'shear- and thrust-planes.'

[2] Compare pp. 10–15 of my "Chem. and Phys. Studies in the Metamorphism of Rocks"; also the quotation from Pfaff in App. ii. Note K. The work of Pfaff is not as well known as it should be by English geologists.

VI.—Rutile in Fireclays—Reply to Major-General MacMahon.

By W. Maynard Hutchings, Esq.

In the June Number of the Geological Magazine, p. 259, Major-General MacMahon deals with my paper on fireclays, etc., and raises, with much fairness, several objections to some of my interpretations and inferences. Some of the criticisms are exactly what I should expect to be made, and were more or less present in my own mind, so that I can well see how they may arise in the minds of others.

General MacMahon appears to have somewhat overstated my views as to the "dynamic" part of the "Metamorphism" in question, owing to taking all I say as equally applying to slates and clays, which is not the case. Thus, the paragraphs he refers to on p. 168 will, I think, plainly show that I distinguish between what has taken place in clays and allied shales, and what has taken place in slates; and passages in my previous paper make this still more evident. I fully hold that slates, such as I refer to, have undergone metamorphism of very "dynamic" nature. And I hold that the beginnings of the same thing went on in the clays and shales, but in a so much less degree that I think General McMahon would fully admit it.

All I claim for the clays is summed up in the sentence at bottom of p. 316 (July Number of last year), where I speak of "the joint action of pressure, warmth, and mineral solutions" as the probable condition of the changes I believe to have taken place. It was only, I think, in replying to Dr. Irving that I used the word "dynamic" at all in connexion with the clays and shales of the Coal-measures. It may be that it is a wrong expression in that combination.

"Dynamic" and "Metamorphic" are dangerous words, perhaps, as they are used so variously by those we look to for light and leading that we hardly know how to use them safely at all.

Such changes as I believe to have taken place in fireclays would not, I assume, be brought about unless the chemical action had been intensified by warmth and *pressure*. That they have been under conditions capable of producing the pressure and warmth is hardly to be doubted.

What are we to call the effects of these things, as distinguished from any ordinary surface-weathering due to simple chemical action? Would "pressure-metamorphism" be allowed to include them? Where does simple chemical action end and "metamorphism" begin?

I at once admit what General MacMahon says as to absence of direct chemical evidence for my views. It is, very unfortunately, a fact that lack of chemical work is felt throughout a great deal of the petrological study that is going on. We have many workers with the microscope, but very few workers with the balance.

In the present instance, however, the nature of the material is such that I do not think the most devoted analyst could obtain very much solid evidence.

The "pestle and mortar" business was not so violent as General

MacMahon seems to infer, the trituration of a clay ("without grinding") partly in water, being a gentle operation. But, violent or gentle, I fail to see that it has any bearing on the fact, on which stress is laid, that after separation none of the larger flakes of mica contain any rutile at all; nor on the conclusion which this fact enables us to draw, in view of what we know of the nature of the materials forming these deposits, that the rutile was not brought into the sediments in the mica, as such.

As regards the fact that the mica of these "complex flakes" containing rutile is orientated in all directions, I do not look upon it as of much importance one way or other. General MacMahon thinks that if the mica is secondary the "molecules of mica would, at the moment of crystallization, surely have followed the laws of crystallization and have arranged themselves in definite order." This is simply an *à priori* supposition as to what *ought* to take place, and can hardly count as an argument. As a matter of fact, observation of mica which is formed as a secondary product in altering felspar, either in crystals of felspar or in fine felspathic ash-material, seems to show that no such definite order is by any means the rule, but rather in most cases quite the reverse. Such, at least, is my experience with materials of this nature of which I am just now making a special study.

It is suggested that the micaceous ilmenite may be the source of the rutile. I do not think examination of the materials could lead anybody to this view. I regard this mineral as secondary, and derived indirectly from the same source as the rutile. I do not know that the transparent and translucent micaceous ilmenite as it occurs in these clays, etc., and abundantly in some slates, has ever been regarded as anything but secondary, nor that any original rock has been suggested whence it could be derived clastically.

Finally, while admitting the full force of some of General MacMahon's criticisms, I venture to think that the subject should be looked at and judged a little more *as a whole*, taking in what I might call outside matters as well as details of the laboratory and the microscope.

Broadly, the matter stands as follows: We have these large and extended deposits of the Coal-measures all apparently similar in nature, derived from similar sources. All the evidence obtainable goes to show that these sources were granites or gneisses. There is not any evidence of any sort to show that any other class of rocks was concerned in supplying the sediment, and I am not aware that any student of them, in the field or in the laboratory, has ever suggested another origin for these beds, either wholly or partially.

A portion of these deposits, the clays, contains these vast numbers of rutile-needles. I suggest that these are of secondary origin, formed *in situ*, and I think they result from the decomposition of biotite-mica, which can be seen to have formed so large an item in the original sediment; because biotite when tested has been found to contain titanic acid, and because it has been observed, under some conditions, to give rise to rutile during its decomposition.

The needles of the fireclays may, however, be secondary and yet not be derived wholly or even in part from the biotite. Some other mineral, sphene for instance, may have been the parent, though, as intimated in my paper, it does not seem so likely.

If the rutile is not secondary, it must have been brought in with the original sediment. I consider it may be looked on as shown that it was not contained as such in the original mica. Whence did it come? Can anybody point to granites or gneisses containing rutile-needles in the abundance required for these extensive deposits, and containing them, moreover, not in the mica, the usual way in which they do occur when seen at all? If, in spite of all the evidence on this head, it is assumed that some other rock and not granite or gneiss was the source of these deposits, then what rock can be suggested which supplied the rutile and the mica? And if it be supposed that this other rock supplied only a *part* of the total materials, but *all* the rutile, then it would be still more interesting to have that rock named. It is easy to say the needles came from elsewhere; but where did they come from?

The subject of the origin of the minerals in these special clays, etc., is of much interest, because it may throw light on the genesis of slates and phyllites, a question which has given rise to much speculation and discussion and received great attention from some of the leaders in petrological work. It is a somewhat difficult subject to deal with, and I would be the last to think that my own suppositions and theories are in any way proved to be more than attempts at an explanation. I only venture to hope that they are not quite out of harmony with the other evidence, which seems so strong, as to the original nature of the deposits; and to think that if they are disputed, if the secondary nature of the rutile is denied, some other definite source for it should be suggested, which should at the same time harmonize with the other facts of the case.

Newcastle-on-Tyne, *June*, 1891.

VII.—On the British Earthquakes of 1889.[1]

By Charles Davison, M.A.,
Mathematical Master at King Edward's High School, Birmingham.

(*Continued from page* 67.)

2. Lancashire Earthquake: Feb. 10, 1889.

Time of occurrence, 22h. 36m.: Intensity, VI. Epicentrum about two miles N.N.E. of Bolton, half a mile west of the village of Bradshaw.

This interesting earthquake affected a district for the most part thickly populated, and the accounts of it are consequently numerous. In the newspapers mentioned below, records are given of observations from 156 towns and villages, situated within a nearly circular area about 55 miles in diameter. Considering the smallness of this area,

[1] The Map will appear with Part III. in the August Number Geol. Mag. —Edit. Geol. Mag.

I suppose that but few previous earthquakes have afforded materials so abundant for the seismologist.

Accessory Shock.—It is probable that at least one slight accessory shock occurred between ten and twenty minutes after the principal shock. Near Blackburn, where the intensity was V., an observer residing at Wilpshire "noticed a slight recurrence of the phenomenon some time afterwards—about twenty minutes." At Prestwich, the intensity was about, or perhaps more than, IV., and there "another but feebler shock took place—perhaps some ten minutes afterwards." The evidence is clearly incomplete, but it seems most probable that both accounts refer to the same shock, which, owing to its very slight intensity, escaped detection elsewhere.

Disturbed Area.—As in the previous earthquake, the places where the principal shock was undoubtedly felt are indicated by small discs, and those where there is good reason for believing that it was not felt by small crosses (×). For the former, I have relied almost entirely on the numerous accounts given in the local press; for the latter, on inquiries made in the district with the view of determining the exact form of the disturbed area. In the immediate neighbourhood of the boundary, the evidence is somewhat scanty, and especially is this the case near Glossop and in the Peak country. Having, as will be seen, no positive evidence from the latter district, the outline in this part cannot be regarded as very exact.

So far as we can judge from the materials collected, the outline of the disturbed area seems to be approximately circular. It will be seen, from the table of intensities given below, that the boundary corresponds to an isoseismal line of intensity less than IV. and greater than III. Outside this area, it is possible that the shock may have been felt, and probable that it would have been detected by any one accustomed to the observation of slight earthquakes. But that it could have been felt as far as Birmingham, as one observer states, is clearly impossible, first, on account of its distance from the rest of the disturbed area, and, secondly, because it was not registered by one of Prof. Ewing's delicate seismographs erected there in my cellar.

The disturbed area, thus defined, is 56 miles from north to south, and 54 miles from east to west; and includes an area of about 2480 square miles.

Time of Occurrence.—The records of the time of occurrence can, unfortunately, lay claim to no great accuracy. So far as I know, the earthquake was registered by no instrument of precision. Excluding, however, all estimates that are admittedly approximate, we have the following definite records. In the immediate neighbourhood of the epicentrum, where the intensity was not less than VI., it was noticed at Bolton at 22h. 35m.; at Turton, at 22h. 36½m. by one account, and 22h. 37m. by another. Outside this range and within the isoseismal of intensity V., we have the records of 22h. 35m. at Farnworth, of 22h. 37m. at Accrington, of 22h. 38m. at Rochdale, Heywood, Bury, and Prestwich; of 22h. 40m. at Tyldesley and Wigan; and, lastly, of 22h. 25½m., clearly wrong if

it refer to the same shock, at Radcliffe. Between this last isoseismal and the boundary of the disturbed area, the following times are given: 22h. 32m. at Rusholme, 22h. 37m. at Greenhalgh, Preston and Meltham; 22h. 43m. at Marbury (near Northwich), and 22h. 45m. at Waterhead and Heaton Chapel.

From this evidence, I have indicated 22h. 36m. as the probable time of occurrence at the epicentrum. If the average surface-velocity of the earth-wave were 1200 feet per second, the shock could be felt at the limits of the disturbed area about two minutes later. But the records in all parts of the area are too discordant to lead to results of any value.

Nature of the Shock.—The descriptions I possess of the nature of the shock are, with one exception, derived entirely from newspapers, and cannot always be closely trusted. As a rule, they are too vague and inexact to justify detailed quotation. There can be no doubt, however, that the character of the shock, especially with regard to the number of vibrations felt, was subject to considerable variation throughout the disturbed area; but this we should be led to expect from Prof. Milne's seismic survey of an area of about nine acres in Tokio.[1] In the present case, it is possible that these variations may in part be attributable to the form and position of the seismic focus.

From a very large number of accounts, I select the following as fairly typical of the different effects observed:—

Bolton: a distinct heavy rumbling for a few seconds, and, mingled with it, a sharp thud like an explosion.

Bury: a rumbling noise, lasting about four seconds, and then a sort of heavy thud, as of some enormous weight dropping on the floor, causing an alarming vibration and a rattling of window-frames.

Prestwich: a shock as of a heavy falling body, which caused the windows to rattle loudly; two or three seconds later, a second thud-like shock of somewhat greater intensity than the first; followed by gentle, but distinct tremors, lasting, perhaps, twenty or thirty seconds more.

Chorley: a noise like the rumbling of heavy waggons on a stone pavement was suddenly heard, and, along with it, a few sharp thuds were felt, accompanied by a vibratory motion of the earth.

Bowdon: a sudden jar was felt, like that produced by a heavy body falling, or by the slamming of a heavy door; then the floor vibrated three times distinctly; and this was followed after a few seconds by another slight vibration, a double one.

Bolton and Bury, it will be seen, are close to the epicentrum of the earthquake, and, at both these places, a single thud, or prominent vibration, was felt. It follows, then, that the double or multiple vibrations observed in other parts of the district cannot have been due to a recurrence of the shock near the same spot as the first; and it seems almost impossible to suppose that they were produced by repetitions of the initial disturbance in different parts of the area. We must conclude, therefore, that the nature of the shock varied

[1] Japan Seism. Soc. Trans. vol. x. pp. 1–36.

with the position of the place of observation relatively to that of the seismic focus, or with the geological structure of the surrounding country.

Now, a single vibration was felt at: (*a*) Blackburn, Bury, Meltham; (*b*) Bolton, Heywood, Ramsbottom, Wigan; a double vibration at: (*a*) Longridge, Prestwich, Rhodes; (*b*) Burnley, Leigh, Marbury, Nelson; three or more vibrations at: (*a*) Chorley, Failsworth, Hollinwood, Middleton, Stretford, Whitefield; and, lastly, a tremulous or continuous vibration at: (*a*) Bowdon, Crompton, Darwen, Manchester, Preston; (*b*) Leftwich, St. Helens, Tottington. The direction of the Irwell fault in the neighbourhood of the epicentrum is approximately N.W. and S.E., and, if lines be drawn through the epicentrum from north to south and east to west, they will divide the disturbed area into four quarters, so that places on the N.W. and S.E. quarters are more nearly in a line with the Irwell fault than places in the N.E. and S.W. quarters. Places in the former pair of quarters are preceded by the letter (*a*) in the above list, and places in the latter pair by the letter (*b*). The results may thus be summarized:—

	a.	*b*.	Total.
Single vibration	3 ...	4 ...	7
Double or multiple vibrations	14 ...	7 ...	21

In the majority of cases, then, double or multiple vibrations were felt at places nearly in a line with the Irwell fault at Bolton; and a single vibration at places whose directions relatively to the epicentrum are nearly at right angles to this direction.

On the vertical component of the motion, very few observations seem to have been made. The movement is described as an upheaval simply, at Chorley, Eccles, Farnworth, Nelson, Oldham, and Wigan; but it is not stated whether the upheaval was followed or preceded by a lowering, or even whether it was accompanied by any such movement in a contrary direction at all. These places are not confined to any particular part of the disturbed area. On the other hand, at the following places, there was first a rise, followed by a lowering of the ground.

Ramsbottom: persons felt as though they had been bodily lifted up and dropped quickly.

Heywood: the sensation was that of a sudden lifting of the dwelling and of as sudden a sinking.

Bury: persons in bed felt as if their beds were raised for a brief time and then allowed to fall.

Prestwich: a jerk up and down, followed by a trembling movement.

These four places, it is important to notice, lie on the north-east side of the Irwell fault, and at only a few miles distance from its intersection with the surface.

Duration.—If, as I believe, the earthquake was due to a slip of the Irwell fault in the neighbourhood of Bolton, and if that slip was confined, not to a point, but to a definite area, we should expect the duration of the shock, as well as its nature, to vary throughout the disturbed area. The duration should of course be greatest at

places in the line of direction of the fault, and shortest at that place situated so that a line joining it to the centre of the seismic focus is perpendicular to the fault-plane. On the other hand, the apparent duration depends on the distance of the place of observation from the focus, and, what is far more important, it depends on the power of the observer to detect the faint initial and final tremors. On this account, chiefly, I think it is not possible to discover any certain law in the distribution of places according to the duration of the shock felt there. In this respect, more may be learnt from the duration of the sounds.

The following are the recorded durations of the shock:—

About 15	seconds.	Farnworth.
,, 6	,,	Blackburn, Bolton.
,, 5	,,	Crosby, Heaton Chapel, Melling.
,, 4	,,	Chorley.
,, 3	,,	Prestwich.
,, 2	,,	Bacup, Cheetham Hill, Crumpsall, Darwen, Meltham, Preston, Waterhead, Whalley.
,, 1	,,	Leftwich, Oldham, Tyldesley.

Intensity.—In the following lists are given the names of the places where it was possible to determine the intensity according to the Rossi-Forel scale.

VI. Bolton, Ramsbottom, Tottington.

V. or VI. Bury, Chesham, Farnworth.

V. Besses, Blackburn, Chorley, Darwen, Golborne, Heywood, Horwich, Hulton Park, Ince, Rochdale, Turton, Whitefield, Wigan.

IV. or V. Blackley, Castleton, Cheetham Hill, Crumpsall, Harpurhey, Littleborough, Middleton, Prestwich.

IV. Accrington, Altrincham, Bolton-by-Bowland, Bowdon, Breightmet, Cheetham, Eccles, Facit, Heaton Park, Lees, Leigh, Leyland, Little Hulton, Manchester, Milnrow, Newhey, Northwich, Whitworth, Withington.

III. or IV. Crosby, Melling, Preston, Whalley.

III. Meltham, Stockport, Wardleworth.

The observations are not sufficiently numerous to enable the isoseismal of intensity VI. to be drawn. All the places where the intensity was V. lie, however, within a well-defined curve which is shown upon the map. This curve is approximately circular. Its dimensions are 22 miles from north to south, and 23 miles from east to west. It includes an area of about 396 square miles. Its centre is half a mile west of the village of Bradshaw and two miles N.N.E. of Bolton.

It will be noticed that, at several places within this curve, the intensity is given as only IV. This may be due to one of two causes. (1) The intensity may really have been more than IV., but the rattling of windows, etc., may have been recorded as the most noticeable effect of the shock; or (2) the intensity may have been different at two neighbouring places within this isoseismal, this being only what we should be led to infer from Prof. Milne's seismic survey of Tokio.

Outside this isoseismal, the places where the intensity is known, though numerous, are scattered over a much greater area, and it is

not possible to draw with any accuracy the isoseismal of intensity IV. As some of these places are not very distant from the line which I have indicated on the map as the boundary of the disturbed area, we may conclude that that boundary represents an isoseismal of intensity less than IV., but yet greater than III. This area, as before remarked, is nearly circular, and it is almost exactly concentric with the isoseismal of intensity V.

Sound-Phenomena.—Records of sound-phenomena come from 49 out of the 156 places where the shock is known to have been felt. These places are indicated as before by small crosses (+) drawn through the spots representing them.

Referring to the map, it will be seen that, with four exceptions, all the places where sounds are said to have been heard lie within an area which is approximately circular. It is of course possible that, outside this area, the sounds might have been heard by observers who were listening attentively or who happened to be more sensitive to such vibrations. This may have been the case in the four exceptions just referred to, but it should be noted that, at each of these places, we have only one record of sound-phenomena, while at all the other places there are either several independent accounts, or else the description is so clear as to be quite free from doubt. The following are the accounts from these four places:—

Bolton-by-Bowland: a rumbling noise.

Longridge: a noise as of a sough of wind.

Greenhalgh: an underneath rushing rumbling noise.

Marbury: a dull rumbling noise.

Whilst, then, the characteristic earthquake-sounds may have been heard outside the curve marked (by a dotted line) upon the map, I believe that that curve includes all the places where the sounds were distinctly and certainly heard.

Assuming this to be the case, two facts are at once evident from an inspection of the map.

(1) The sounds were not heard so far as the vibrations of longer period were felt. They were confined to a nearly circular district, 29 miles in diameter, and about 636 square miles in area.

(2) The sound-curve is not concentric with the two isoseismal curves drawn upon the map. At its northern limit, it nearly touches the isoseismal of intensity V.; but, at the other end, it passes about six miles to the south-west of this line. Its centre is about $3\frac{1}{4}$ miles S.S.E. of the epicentrum; but this determination cannot be regarded as so accurate as in the case of the Edinburgh earthquake, for it is not checked by a knowledge of places where the sound was certainly not heard.

This second conclusion—the excentricity of the sound-area—is also borne out by the following table. The earthquake-sounds are recorded as having been heard:—

Within the isoseismal of intensity V. at 33 out of 72 places, or in 46 p.c. of the whole.						
Between this isoseismal and the sound-curve at	12	„	25	„	48	„
Between the sound-curve and the boundary of the disturbed area at	4	„	41	„	10	„

Throughout the area in which they were heard, the sounds varied greatly both in nature and duration; and, in most cases, the variation seemed to have depended on the position of the place of observation. The exact duration is not often explicitly stated. It is given at about four seconds at Bury, four or five seconds at Chorley, about two seconds at Bacup, several seconds at Altrincham, and a few seconds at Bolton and Middleton. Something may, however, be inferred from the numerous accounts that have been given of the phenomena.

The following descriptions seem to indicate that the sound was more or less sudden and transitory:

(*a*) Cheetham: as of a heavy body falling in the street.
Darwen: (1) closely resembling that made by a door being violently shut; (2) like the fall of a signal-post.

(*b*) Accrington: like the thud caused when snow falls from the roof.
St. Helens: a rumble as of a heavy thud on the gable end of the house.
Tyldesley: as if some heavy weight had fallen in the room below.

In the next group, the sounds were apparently more prolonged:

(*a*) Chorley: like the rumbling of heavy waggons on stone pavement.
Eccles: there was a noise as of very heavy snow sliding down the roof, and as it passed the S.S.W. corner there was a sound as if many tons of snow had fallen on the ground.
Heaton Chapel: resembling a vehicle in the street.
Leyland: such that a cabman thought his horse had run away.
Lower Broughton: like the suppressed roaring of wind entering a gorge.
Manchester: a rustling noise as of wind playing among loose paper.
Moston: a noise attributed to a colliery explosion.
Prestwich: like a heavy but distinct roll of thunder.
Sough: resembling that of a horse which had got loose in its stall.

(*b*) Bacup: like that of a road-engine.
Hulton Park: as if a carriage were driving up to the door.
Leigh: as of heavy waggons rolling rapidly along the street.
Orrell Post: like that of a heavy conveyance passing in the heavy snow which had fallen during the day.

Now, in both lists, all places preceded by the letter (*a*) are situated in the N.W. and S.E. quarters formed by drawing lines from north to south and east to west through the epicentrum; and those preceded by the letter (*b*) are in the N.E. and S.W. quarters. These results are summarized in the following table:

	a.	*b*.	Total.
Sounds of short duration at ...	2 ...	3 ...	5 places.
Sounds of long duration at ...	9 ...	4 ...	13 ,,

We may conclude, then, that, on the whole, the duration of the sound was greater at places near the line of the Irwell fault than at places more remote from it.

Miscellaneous phenomena.—As it is somewhat unusual for earthquakes to be felt strongly, if at all, in pits and excavations, it is worth mentioning that the Lancashire earthquake was noticed in mines in several instances. In one of the collieries in the Ince district, near Wigan, the shock was felt so distinctly by a party of men, that a careful inspection was made by them to see if any accident had happened. In the Pendlebury Colliery, "at about 10.30 p.m. the colliers on No. 9, East side, Rams Mine, came out of their working places to the engine-house and reported to the fireman that they had heard a rumbling noise in the roof of the working places." At Agecroft Colliery, "only one man noticed anything. He was in No. 2 pit, and when he heard the sound he went out to the pony road to see if it had been caused by a fall of dirt."[1]

Stonyhurst Observatory is about 19 miles north of Bolton, outside the isoseismal of intensity V., but still some miles within the boundary of the disturbed area. As it seemed possible that the magnetic instruments might have been affected by the earthquake, I wrote to the late Father Perry on the subject. The letter arrived during his last absence from England; and, after his death, Mr. W. C. Cameron was good enough to inform me that no trace of the earthquake was shown by the magnetic curve of February 10. This result is curious, considering that, shortly after several recent earthquakes, magnetic instruments in distant observatories, far beyond the range of the sensible shock, have been perceptibly affected. The cause of these disturbances is not certainly known, but is probably due to the mechanical action of the shock, the masses of the two arms of the instrument being made unequal, in order that it may rest horizontally.[2] If this be the case, the absence of any noticeable disturbance at Stonyhurst may have been due to the fact that the line joining it to the epicentrum of the earthquake is so nearly in the direction of the magnetic meridian.

Position of the Seismic Focus and Geological Relations.—The faults of the Bolton district are, with a few small exceptions, arranged in two systems, one running N.N.W. and S.S.E., and the other nearly E. and W. Prominent among the former series is the great Irwell Valley fault. This important dislocation, which is shown upon the map of the earthquake, has been traced for a distance of more than twenty miles, from near Poynton in Cheshire to about three miles north-west of Bolton. Throughout its whole course the downthrow is towards the north-east. The amount of its throw is considerable, being 1050 yards at Farnworth, about three miles south-east of Bolton.[3]

Now, as we have seen, both isoseismal lines are approximately circular and almost exactly concentric. The centre of the smaller curve (intensity V.), which is the more accurately drawn, is about two miles N.N.E. of Bolton, or half a mile west of Bradshaw; and

[1] The last two accounts are quoted from a note read by Mr. J. Knowles before the Manchester Geological Society on March 12, 1889.

[2] Geol. Mag. Dec. III. Vol. II. pp. 210–211.

[3] E. Hull, Geology of the Country round Bolton, Lancashire (Geol. Surv. Mem.).

we may take this point as indicating very nearly the position of the epicentrum, which is therefore close to the intersection of the Irwell fault with the surface, and on the downthrow side of it.

The centre of the sound-area, as drawn upon the map, lies, however, about $3\frac{1}{4}$ miles S.S.W. of the epicentrum, and therefore to the south-west of the line of the Irwell fault. But since, as before remarked, there may be a slight error in this determination, and since the minor faults of the district are excluded, by their position with respect to the epicentrum, from any relation with the earthquake, I think we may with some probability connect the origin of the earthquake with the Irwell fault.

There is, however, other evidence bearing on the point. (1) It has been shown that, as a general rule, in the neighbourhood of the continuation of the Irwell fault near Bolton, the number of vibrations was greater than elsewhere, and the sound-phenomena were also more lasting; and this is what we should expect if the seismic focus were a plane whose strike is N.W. and S.E. (2) The maximum intensity was recorded at Tottington, about two miles east of the epicentrum, and this, it will be seen, is not far from the point where the perpendicular to the fault-plane through the centre of the seismic focus meets the surface of the earth. Other conditions being the same, it is evident that the intensity should be greatest in the neighbourhood of this point.

The inclination of the Irwell fault near Bolton is 28° from the vertical, according to the horizontal section (sheet 67) of the Geological Survey, which is drawn across the fault. If this inclination be approximately constant to within a few miles from the surface, it follows that the centre of the seismic focus must be at a depth of about $3\frac{3}{4}$ miles.

Again, as in the case of the Edinburgh earthquake, the movement at four places (Bury, Heywood, Prestwich, and Ramsbottom) on the downthrow side of the fault, was upwards first and then downwards. There are no records of a contrary movement on the upthrow side of the fault; but the evidence, so far as it goes, indicates that the earthquake was probably caused by a slip which slightly increased the throw of the fault.

Lastly, that the distance in a horizontal direction over which the slip extended was very short, may be inferred: (1) from the approximate circularity of the isoseismal lines, and (2) from the short duration of the earthquake-shock and the accompanying sound-phenomena. Since the duration is rarely mentioned as being more than a few seconds, it is possible that this distance was not much greater than a mile.

The whole evidence being taken together, the following conclusions seem to me probable: (1) the Lancashire earthquake was caused by a slip of the Irwell fault, a few miles from its north-western extremity as traced upon the map: (2) the horizontal length of the slip-area was short, possibly less than a mile: (3) the slip resulted in an increase of the throw of the fault: and (4) the slip must have extended upwards to within a short distance from, if

not quite up to, the surface, the amount of the slip near the surface being of course extremely small.

Origin of the Sound-phenomena.—The observations on the sounds that accompanied the Edinburgh and Lancashire earthquakes lead to an important result, throwing light on the origin of these vibrations and the part of the focus from which they proceed. I will now point out briefly what I believe to be the origin of these sounds, reserving a more complete discussion of the question for a subsequent paper in which other facts bearing on the problem may be conveniently brought together.

In both earthquakes, the shock and sound, we have reason to believe, were caused by slipping along well-known faults, the foci of the sounds being nearer the surface than the foci of the corresponding shocks. In both, also, the area over which the slip took place must have been very limited in extent: and, while the amount of the slip may have been greatest near the centre of this area, it must certainly have died away towards its upper and lateral margins.

Now, the seismographic records recently obtained by Prof. Milne and others in Japan show that earthquakes usually begin with a series of tremors very small in amplitude and very rapid in period, from six to eight occurring every second, but becoming slower before the shock takes place. These may last for many seconds or even several minutes. Following, and continuous with, them come the sensible vibrations, of larger amplitude and longer period, about three to five occurring in every second. One or more of these, attaining a still greater amplitude and longer period, of one or two seconds each, constitute what are generally known as the principal shock or shocks. The earthquake closes with vibrations of smaller amplitude, but which have a period so long that no record of them can be obtained. The earliest tremors, on the other hand, are not registered on account of the smallness of their amplitude, and, in all probability, as Prof. Milne suggests, the "minute movements which have been recorded are the continuation of still smaller and more rapid movements which . . . . have never yet been rendered visible." It is to these supposed rapid vibrations which form the front portion of an advancing earthquake, that Prof. Milne attributes the origin of the earthquake-sounds.[1] We may conclude from these observations that, initially at any rate, the period of the vibrations increases and decreases with their amplitude.

Now, from different parts of the area over which a fault-slip takes place, there must proceed vibrations differing greatly in amplitude, and therefore also in period. From the central portions of the slip-area will come, as a rule, the vibrations of largest amplitude and longest period; while, from the margins there will proceed minute vibrations of a period so short that they may be perceptible only as sound. The position of the line separating the marginal and central parts of the slip-area will depend only on the amplitude of the vibrations corresponding to the period of the lowest sound that

[1] Prof. J. Milne, Japan Seismol. Soc. Trans. vol. xii. (1888), pp. 53–62 and 107–8.

can be heard; it will not at all depend on the amount of the slip at the centre of the area, *i.e.* it will be independent of the *intensity* of the shock.

I shall endeavour to show, in a later paper, that this theory of the origin of earthquake-sounds accounts satisfactorily for all their phenomena, so far as they are known to us. For the present, it will be sufficient to point out that it explains (1) the fact that the sound-area is not concentric with the disturbed area, and the sound-focus is nearer the surface than the rest of the seismic focus; and (2) the fact that, in great earthquakes, the sounds are only heard within a comparatively small area immediately surrounding the epicentrum.

Authorities.—"Accrington Gazette," Feb. 16; "Accrington Times," Feb. 16; "Altrincham Division Advertiser," Feb. 15; "Blackpool Times," Feb. 13; "Bolton Chronicle," Feb. 16; "Bradford Observer Budget," Feb. 16; "Burnley Express," Feb. 16; "Bury Guardian," Feb. 16; "Chorley Standard," Feb. 16; "Craven Herald" (Skipton), Feb. 16; "Darwen Post," Feb. 16; "Halifax Courier," Feb. 16; "Heywood Advertiser," Feb. 15; "Huddersfield Chronicle," Feb. 16; "Lancaster Guardian," Feb. 16; "Leeds Mercury," Feb. 12; "Leigh Chronicle," Feb. 15; "Liverpool Mercury," Feb. 12; "Macclesfield Chronicle," Feb. 15; "Macclesfield Courier," Feb. 16; "Manchester Examiner," Feb. 12; "Manchester Guardian," Feb. 12, 13; "Middleton Guardian," Feb. 16; "Northwich and Knutsford Chronicle," Feb. 16; "Oldham Standard," Feb. 16; "Preston Guardian," Feb. 16; "Rochdale Observer," Feb. 13, 16; "Wigan Observer," Feb. 13.

Nature, vol. 39, p. 376; T. R. H. Clunn, The earthquake in Lancashire, Nature, vol. 39, p. 390; J. Knowles, The earthquake shock of February 10th, 1889, Manchester Geol. Soc. Trans., vol. xx. pp. 155–157.

For other information I am indebted to the kindness of the following gentlemen: the Secretaries of the Geological Societies of Liverpool and Manchester, Mr. W. C. Carlisle (Stonyhurst Observatory), Mr. B. Hainsworth (Rossall), Mr. G. Hartnup (Liverpool Observatory, Bidstow) Prof. E. Hull, F.R.S., and Mr. Isaac Roberts, F.R.S. (Maghull).

(*To be continued.*)

NOTICES OF MEMOIRS.

On the Young of *Baculites compressus*, Say. By Amos P. Brown. (Proc. Acad. Nat. Sci. Philadelphia.)

At the meeting of the Academy of Natural Sciences of Philadelphia on March 10th, Mr. Amos P. Brown described the young of *Baculites compressus*, Say, recently discovered by him in some Cretaceous marl from the vicinity of Deadwood, South Dakota. Associated with them in the same material were several species of *Baculites*, *Scaphites*, and *Inoceramus*. The young *Baculites* varied in length from 1 to 3 cm., with a diameter of 0·4 to 2 mm.

The shell originates in a spiral consisting of from two to two and one-half slightly overlapping whorls, and ranging in diameter from 0·8 to 1 mm.; thence it extends in a straight line, tangent to the spiral, or sometimes slightly reflexed. The straight portion of the shell rapidly increases in diameter from 0·38 to 0·40 mm. at the spiral, to about 1·5 to 2 mm. at 2 cm. length. Some of the specimens were entire, and showed that the body-chamber occupied about one-half the length of the shell. The siphuncle is eccentric, and was found to lie near the outer margin of the spiral.

The species was determined from an examination of the suture-line which was traced from the simple form of the very young shell, through forms of gradually increasing complexity, up to the typical suture of the adult of *Baculites compressus*, Say.

That this spiral portion should have hitherto escaped observation can be easily accounted for by its small size and fragile character. In all probability it was broken off long before the shell had attained adult size, and is therefore to be met with only in shells which were preserved in their immature condition.

The description is given in the Proceedings of the Academy, 1891, part i. pp. 159–160, and is accompanied by figures of the young *Baculites*, and of a series of their suture-lines showing the gradual development from a comparatively simple form to the typical suture of the adult of *Baculites compressus*, Say.

REVIEWS.

I.—Professor Osborn on the Molars of the Perissodactyla.

MANY of our readers are probably acquainted more or less fully with Professor H. F. Osborn's elaborate and interesting researches into the nature of the primitive plan on which the molar teeth of mammals are constructed, summaries of which have appeared from time to time in several English scientific journals. The result of these researches was to show that this primitive type of tooth was of the so-called 'tritubercular' form. It may not be out of place to remind our readers that such a tritubercular tooth in the upper jaw is composed of an inner tubercle, or *protocone*, and of two outer tubercles respectively designated *paracone* and *metacone*. By the addition of a second inner tubercle—the *hypocone*—and of two intermediate ones, known as the *protoconule* and *metaconule*, such a tritubercular molar becomes converted into the sextubercular one occurring in many Ungulates.

Professor Osborn has recently been engaged in investigating how the more complex molars of the specialized Perissodactyles have been evolved from this sextubercular type; the results of this investigation being published in a memoir issued under the auspices of the Museum of Harvard College.[1] Since these results are of considerable morphological importance, and are especially interesting to palæontologists, a short *resumé* of them may be acceptable to our

[1] Bull. Mus. Comp. Zool. Harvard Coll. vol. xx. pp. 87, *et seq.* (1890).

readers. For this purpose Prof. Osborn has been good enough to place at our disposal *clichés* of some of the figures illustrating his memoir; which figures, with some of our own, we reproduce.

It may be well to premise our observations by stating that the upper molars of the Perissodactyles are constructed on what is known as the *lophodont* type; this modification consisting of an outer wall, connected with two more or less nearly complete transverse crests inclining backwards. The simplest modification of this type of molar is to be found in the little *Hyracotherium* of the

FIG. 1.—Left upper premolar and molar teeth of *Anchitherium* (*Mesohippus*) *bairdi*. *pr.* protocone; *pp.* posterior prominence.

London Clay; but those of the generalized Horse-like animal known as *Anchitherium* (including *Mesohippus*) are but little more advanced. Of this genus we reproduce two of Prof. Osborn's figures; one (Fig. 1) showing the whole series of cheek-teeth, and the other (Fig. 2) a single molar. One species (Fig. 1) is a smaller and simpler form, in which the tubercle marked *pp* is less developed than in the larger species (Fig. 2). In these teeth there is no difficulty in recognizing the six elements of the sex-tubercular tooth, all of which are indicated by letters in Fig. 2, with the exception of the metaconule, which is the unlettered ridge occupying the middle of the tooth, immediately below the *m*. The four remaining elements, lettered *a*. *m*. *p*. and *pp*., have been developed from the basal cingulum surrounding the molar of *Hyracotherium*; and, accordingly, have nothing whatever to do with the primitive foldings of the crown. This origin is very important with regard to the tubercle marked *pp*. Thus in Fig. 2 this is so strongly marked as to suggest an origin like the other main tubercles; but its commencement as a slight swelling of the cingulum is clearly shown in Fig. 1. To the elements respectively marked *a*. *m*. and *pp*., Professor Osborn applies the terms *anterior pillar*, *median pillar*, and *posterior pillar*; stating that the latter term is taken from the writer of this notice, by whom it is said to have been proposed for the teeth

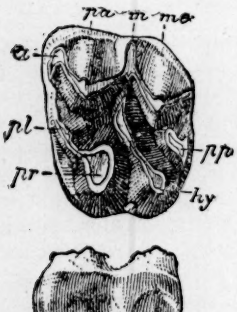

FIG. 2.—Grinding and outer aspects of a left upper molar of *Anchitherium longiciste*. *pr.* protocone; *pa.* paracone; *me.* metacone; *hy.* hypocone; *pl.* protoconule; *a.* anterior ridge; *m.* middle ridge; *p.* posterior ridge; *pp.* posterior prominence.

of the Horses. To this statement we must take the present opportunity of objecting. The term *posterior pillar* (although it has been

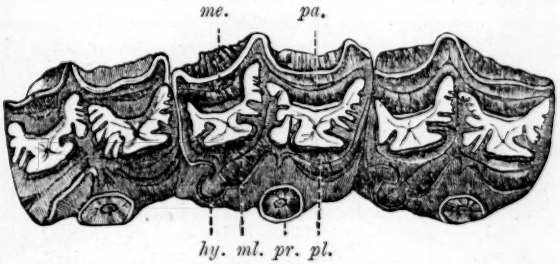

FIG. 3.—Three right upper cheek-teeth of *Hipparion*. *ml.* metaconule; letters as in Fig. 2.

subsequently employed by the writer) was first proposed by Prof. Huxley ("Anatomy of Vertebrates," p. 354, 1871), and was applied to the tubercle in the molars of the Horses marked *hy.* in Figs. 3 and 4 (these figures are unfortunately from the opposite side of the jaw to those figured by Prof. Osborn). This tubercle corresponds with the one similarly lettered in Fig. 2 (the hypocone); and has, therefore, nothing whatever to do with the one marked *pp.* Under these circumstances we propose to substitute the term *posterior prominence* for the tubercle *pp.* Again, the term *anterior pillar* was proposed by Prof. Huxley, and adopted by the writer for the tubercle marked *pr.* (the protocone) in Figs. 2, 3; and it is, therefore, quite inadmissible to apply it to *a.* in Fig. 2. Under these circumstances we would suggest that the vertical ridges marked *a. m.* and *p.* in Fig. 2 (which become still more prominent in the teeth of the Horses), should respectively be designated the *anterior ridge, middle ridge,* and *posterior ridge.*

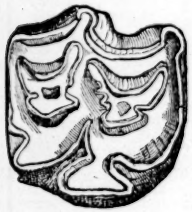

FIG. 4.—Right upper molar of *Equus stenonis*; letters as in Fig. 3.

The figures sufficiently indicate how the equine molar has been evolved from that of *Anchitherium* by the development of one projection from the protoconule to join the metaconule, and of another from the latter connecting it with the posterior prominence, and thus with the metacone; the lengthening of the crown and the filling of the valleys with cement being a concomitant process. The result of this evolution has been to form complete crescents in the equine molar, of which the hinder one is always connected with the hypocone (*hy.*); while the anterior one may either be connected with the protocone (*pr.*), as in *Equus,* or completely isolated, as in *Hipparion.* This has also entailed the division of the median and posterior valleys of the *Anchitherium* molar into an inner and an outer moiety.

The remaining type of Perissodactyle molar we have to notice is that of the Rhinoceroses (Fig. 5). Here Prof. Osborn has intro-

duced three convenient new descriptive terms, which will be equally applicable to the molars of other Perissiodactyles. It appears that the outer wall of the Rhinoceros molar is formed by the union of the primitive paracone (*pa.*) with the metacone (*me.*), to which is added the anterior ridge (*a.*); and for this outer wall the term *ectoloph* is suggested. The anterior transverse crest, formed by the union of the paracone (*pa.*), protoconule (*pl.*), and protocone (*pr.*) is termed the *protoloph*; while for the posterior transverse crest, compounded of the metacone (*me.*), metaconule, and hypocone (*hy.*) we have the name *metaloph*.

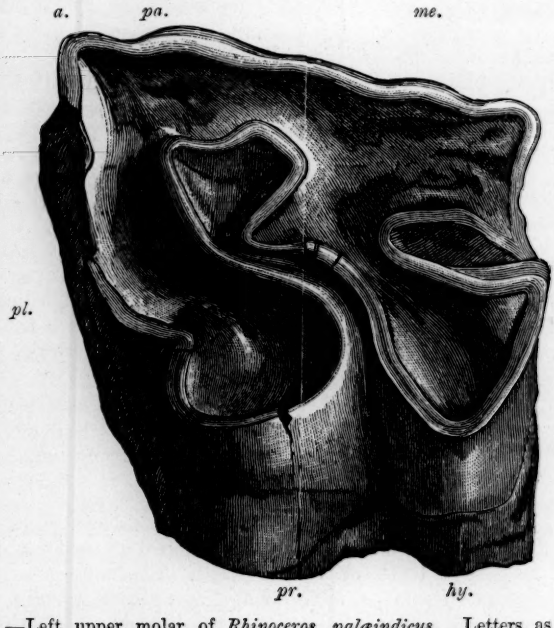

FIG. 5.—Left upper molar of *Rhinoceros palæindicus*. Letters as in Fig. 2. *a.* to *me.* is the ectoloph; *a.* to *pr.* the protoloph; and *me.* to *hy.* the metaloph. The process projecting from the metaloph into the median valley is the crotchet.

In the primitive Rhinoceros molar the three elements—anterior ridge, paracone, and metacone—of the ectoloph are represented by distinct vertical ridges; but some or all of these tend to disappear with the increasing flatness of the outer surface of this part of the tooth as specialization increases. And it is somewhat noteworthy that while in the most specialized types of teeth, like those of the Indian Rhinoceros, of the two African species, and of the extinct Woolly Rhinoceros, the development of the paracone is less marked than in the more generalized Sumatran and Javan species; yet the ridge marking the metacone is decidedly more pronounced. Although there is no distinct representative in the Rhinoceros molar of the

median ridge (Fig. 2, *m.*) of the equine tooth, yet we think that this is foreshadowed in the former by the flexure which often occurs in the ectoloph between the para- and meta-cones.

In referring to the secondary enamel-folds which grow from the three compound elements of the Rhinoceros molar into the median valley (the large fissure penetrating Fig. 5 from the inner side), it is to be regretted that Prof. Osborn has again displayed some carelessness in quoting previous work. Thus he states, "These secondary elements consist, first, of three folds projecting into the median valley, one from the ectoloph, the *crista*; one from the protoloph, the *crochet*; one from the metaloph, the *anticrochet*;" his figures being lettered accordingly. Now the fact really is that it is the *crotchet* (as we prefer to spell the word) which projects from the *metaloph*, as shown in Fig. 5, while the *anticrotchet*, when present, is carried by the *protoloph*. The crotchet corresponds, in fact, precisely in position with the projection arising from the protoconule of the protoloph of the equine molar (Figs. 3, 4); and when reaching across to the metaloph divides the median valley into an inner and outer moiety precisely after the equine manner; the only difference being that while in the Rhinoceros the two divisions of the valley remain open, in the Horse they are completely filled with cement. (In Fig. 3 the cement filling the 'island' formed by the outer part of the median valley is drawn white, while that in the outer part of the same—between *pr.* and *ml.*—is made dark and crossed by vertical lines; this difference is owing to the staining of the fossil, but in a recent Horse's both would be pale buff.)

In conclusion, it may be observed that, while it may be convenient in monographs to retain for the description of the molars of the various families of the Perissodactyla the terms commonly in use, yet the importance and interest of being able to trace the precise morphological equivalents of the elements of all these teeth, not only to one another, but also to the primitive type of mammalian tooth-structure, can scarcely be overrated. To a certain degree as regards the former part of this task, and altogether as regards the latter part, zoologists and palæontologists are indebted to the labours of Prof. Osborn.

R. Lydekker.

II.—Geological Survey of Illinois, A. H. Worthen, Director. Volume VIII. Geology and Palæontology. Edited by Josua Lindahl, Ph.D., State Geologist. *Geology*, by A. H. Worthen. *Palæontology*, by A. H. Worthen, Charles Wachsmuth, Frank Springer, E. O. Ulrich and Oliver Everitt. With an Appendix (*Biography, Bibliography* and *General Index* to Volumes I.–VIII.). Text. Published by authority of the Legislature of Illinois, July, 1890. 8vo. pp. xiii. 758 and 151, 1 Portrait, 1 Map. Plates I.—LXXVIII. and Explanations, bound in a separate volume.

WITH this massive volume and the Plates accompanying it, the series of Reports on the Geology and Palæontology of Illinois, which have appeared at various intervals since 1866, is

finally closed. These Reports, second only in importance to those of the State of New York, have very materially contributed to the advancement of Geological Science on the North American Continent, and they serve as a permanent memorial in honour of Prof. A. H. Worthen, the State Geologist, by whom they have all, with the exception of the closing volume, been edited. To a great extent the present volume was also prepared under his superintendence, but death removed him from his labours, whilst the work was going through the press. The final editing was completed by Dr. J. Lindahl, the successor of Prof. Worthen on the State Survey, and it is very appropriate that a sketch of the life and scientific work of the late Director should have been included in the contents of the final volume. Dr. Lindahl has further contributed a General Index to the entire series, which will prove of considerable assistance to all palæontologists and others who may require to refer to it.

The present volume commences with a chapter on the Drift Deposits of Illinois by Prof. Worthen. From this it appears that they vary in thickness from 10 to 350 feet, and they often extend below the present drainage level of the streams and rivers. The Drift materials are thickest in the central portions of Illinois, they thin out to the south of the State, and, with the exception of the Loess, they all disappear before reaching the Ohio River. The lowest beds consist of stratified sands and clays filling up pre-glacial valleys, over these a layer, 2 to 13 feet thick, of peaty soil filled with plant remains, is usually present. This old soil is covered by Boulder-clays, from 20 to 100 feet in thickness, and these again are partially covered by modified drift and loess containing the bones of *Mastodon*, Mammoth, *Bos*, *Castoroides*, etc. In some places there appears to be a deposit of Boulder-clay material beneath the peaty soil as well as overlying it. The preservation of this soft peaty soil leads Prof. Worthen to the conclusion that the Boulder-clay must have been produced by floating ice rather than by glaciers.

In a second chapter the same author gives the results of numerous borings for coal and in search of natural gas and oil, but though some of the borings have been carried to a depth of over 2300 feet, penetrating nearly to the base of the stratified rocks in the region, no economical results as regards gas and oil have been obtained.

The second part of the volume is devoted to Palæontology, and the first section contains descriptions of fossil Invertebrates by A. H. Worthen. Numerous species of Corals, Crinoids, Molluscs, etc., for the most part from different divisions of the sub-Carboniferous, are treated, but they do not seem of special importance. The discovery of a new species of *Ascoceras*, *A. Southwelli*, from the Niagara Limestone, is of interest.

In section ii. Messrs. Wachsmuth and Springer describe new species of Crinoids and Blastoids from the Lower Carboniferous of Le Grand, Iowa, and from the Niagara Group of West Tennessee. Not infrequently the stems of these Crinoids are preserved intact, and in almost all cases they taper distally to a fine point and give

off rootlets or cirrhi in all directions, from which the authors conclude that these and other Palæocrinoids probably lived a semi-free existence, like recent forms of *Pentacrinus*, either anchored by the cirrhi to the soft oozy sea-bottom, or at intervals attached to foreign bodies. The new species belong to the following genera: *Actinocrinus, Megistocrinus, Dorycrinus, Rhodocrinus, Platycrinus, Dichocrinus, Graphiocrinus, Scaphiocrinus*, and *Taxocrinus*. A specimen of a new Blastoid, *Orophocrinus fusiformis*, retains a portion of the stem and also the pinnules.

In sections iii. iv. and v. Mr. E. O. Ulrich (in part nominally associated with Mr. O. Everett) treats of American Palæozoic Sponges. The author's study of this group of fossils is evidently limited and imperfect, shown more particularly when he ventures to express opinions of his own as distinct from those procured from the writings of and correspondence with experienced authorities. Thus, for example, he believes that there are homological affinities between some of the new Silurian lithistid sponges described and the Stromatoporoid genus *Actinostroma*! It is generally acknowledged that the correct determination of fossil sponges is beset with special difficulties owing to mineral changes which they have undergone, and those which Mr. Ulrich has attempted to describe are no exceptions to the rule. This fact will, perhaps, account for the extraordinary type of spicule which is figured as characteristic of a proposed new family, the Aspidellidæ (p. 224, fig. 6), in which four smooth simple rays are thrown off in pairs from the ends of what is styled a profoundly elongated node or horizontal central bar!

Of Devonian and Carboniferous forms, the author describes four new genera: *Hystriospongia*, a tetractinellid; *Batospongia*, either a lithistid or a calcisponge; *Belemnospongia*, a monactinellid, but which, if correctly delineated as having acerate spicules connected by short transverse processes, must be quite distinct from this sub-order, and *Syringelasma*, a lithistid. At the lower horizon of the Trenton limestone, at a single locality in Illinois, a considerable number of fossil sponges have been obtained, which retain their outer forms and canal structure, but their original siliceous skeletons have been replaced by calcite. These sponges have been arranged in several new genera, and placed as a distinct lithistid family of the Aspidellidæ on the ground that the form of their spicules differs from that of any previously described sponge. From an examination of some of these forms, we believe their spicular structure is closely similar to that of *Astylospongia* and that they can be included in the Anomocladina family. The two principal genera are *Zittelella* and *Anthaspidella*, which apparently differ only in the fact that the former is simple and the latter compound. The former genus, moreover, seems to be synonymous with the previously described *Steliella*,[1] Hinde, which has been overlooked by the author. Another new genus, *Edriospongia*, seems to be based on a portion of a specimen of *Aspidella*. *Strotospongia* and *Dystactospongia* are included with Calcisponges; but the former of these is almost

[1] Canadian Record of Science, July, 1859, p. 395.

certainly siliceous; the character of another new genus, *Camarocladia*, is very indefinite.

A single Stromatoporoid is placed under *Actinostroma*? *Trentonense*, and its minute structure is stated to resemble that of the Aspidellid sponges!

Section vi. treats of Palæozoic Bryozoa, by E. O. Ulrich, and forms by far the most important subject in the volume, occupying 403 pages of text and 49 quarto plates. On this group we can readily recognize that the author is on safer ground than when treating of fossil sponges, for he has thoroughly investigated the microscopic structure of nearly all the American species, and prepared nearly 4000 thin sections for this end. His descriptions and figures furnish an immense addition to our knowledge of the minute structural details of the various forms, and they afford a very striking instance of the advantage to be gained by the systematic microscopic study of all the forms in a single group of organisms. A chapter is devoted to the general and comparative structure of Palæozoic Bryozoa, and the views stated will be generally accepted; some exceptions, however, might be taken to the assertion that the acanthopores were all originally hollow, though it is acknowledged that their summits generally appear solid at the surface. Allied to the acanthopores are the so-called median-tubules, and the hollow structure of these is equally doubtful.

As regards the classification of the Palæozoic Bryozoa, Mr. Ulrich adopts the plan of extreme division, and a great number of new families and genera are introduced on grounds which appear quite insufficient. It is very doubtful if either of the proposed suborders, Trepostomata, Ulrich, and Cryptostomata, Vine, can be logically established; and numerous objections could be taken to many of the family and generic divisions which have been made. That many of the new genera appear to conflict seriously with those already established is only what may be looked for in a work hailing from the United States, and much of this apparent synonymy could probably be avoided by a franker recognition of the characters of the older types and a less hostile attitude towards the authors of these. In spite of these drawbacks the work of Mr. Ulrich on the American fossil Bryozoa merits the warm appreciation of all palæontologists, and affords a safe basis for future students and systematists to build upon. G. J. H.

III.—Catalogue of the Fossil Cephalopoda in the British Museum (Natural History), Part II. containing the remainder of the Nautiloidea. By Arthur H. Foord, F.G.S. (London, 1891.) Printed by order of the Trustees, and sold by Kegan Paul, Trench, Trübner & Co. 8vo. pp. xxviii. and 408, with a large folding Table and 86 Woodcuts.

WE gladly welcome Part II. of Mr. Foord's Catalogue of the Fossil Cephalopoda in the British Museum (Natural History). Two years have elapsed since the completion of Part I., containing part of the suborder *Nautiloidea*, consisting of the families *Ortho-*

ceratidæ, Endoceratidæ, Actinoceratidæ, Gomphoceratidæ, Ascoceratidæ, Poterioceratidæ, and *Cyrtoceratidæ.* The present volume contains the remainder of the *Nautiloidea,* consisting of the families *Lituitidæ, Trochoceratidæ,* and *Nautilidæ.* The family *Bactritidæ* was inserted by the author in the Table of the Nautiloidea which he gave in his Introduction to Part I., but in the Introduction to Part II. he states, "On reconsidering the question of the affinities of *Bactrites* (the sole representative of the family *Bactritidæ*), in the light of the investigations of Branco and Hyatt, I am now prepared to accept the systematic position assigned to it by those authors—that is, at the commencement of the Ammonoidea. *Bactrites* will therefore be dealt with in Part III. instead of in this volume."

From a Table appended to the Introduction giving the "Distribution of Genera and Subgenera of Nautiloidea described in Parts I. and II. of the present Catalogue, with the number of species assigned to each," it is seen that there are in the Museum 573 species included in the Nautiloidea, 459 of which are Palæozoic, 93 Mesozoic, and 21 Cainozoic. The Palæozoic species are distributed in 29 genera and 6 subgenera, the Mesozoic in 4 genera and 2 subgenera, and the Cainozoic in 2 genera and 1 subgenus.

The author commences the present volume with the Lituitidæ, which family includes the genera *Lituites, Ophidioceras,* and *Ancistroceras*; the last-named genus does not appear to be represented in the National Collection. The first is represented by the well-known *L. lituus* from the Orthoceras limestone of Sweden, and possibly by one British species. To the genus *Lituites,* J. de Carle Sowerby referred several British species which he described in Murchison's 'Silurian System.' Prof. Blake, in his Monograph of the British Fossil Cephalopoda, Part I., retained in this genus only one of these species, viz. *ibex,* and associated with it in the same genus, though with a query, some specimens which he identified with *Trochoceras arietinum* of Barrande. Mr. Foord replaces this latter species in *Trochoceras,* and refers the *ibex* of J. de C. Sowerby to *Lituites* with a query, remarking that "the presence of *Lituites* in the British rocks must, for the present, be considered as very far from satisfactorily determined."

Ophidioceras, which is known only in the Silurian of England and Bohemia, was regarded by its founder Barrande as a subgenus of *Lituites,* but Prof. Blake regards it as a distinct genus only "remotely related" to *Lituites,* while Hyatt places it near to *Ascoceras.* Mr. Foord considers it to be a distinct genus "whose alliance seems to be clearly with *Lituites.*"

In *Trochoceras,* the genus representing the *Trochoceratidæ,* and ranging from the Cambrian to the Devonian, the author describes two new Silurian species, each founded upon a single specimen. The one, *Trochoceras boreale,* collected by Captain Inglefield in the Silurian rocks at Wellington Channel, Arctic America, is a smooth species, and is stated to be "much larger than any of those of the Niagara rocks of North America that come at all near to it." The other, from the Woolhope Limestone, at Hay-Head, East of Walsall,

Staffordshire, is a distinctly ribbed species most nearly allied to *Trochoceras mirandum*, Barrande.

In Vol. I. of the Palæontology of New York, Hall described and figured under the name *Lituites undatus* specimens which, according to Mr. Foord, belong to two distinct genera. Prof. Hyatt referred them to the genus *Trocholites*, but Mr. Foord considers that some belong to the genus *Trochoceras*, and to these he has given the name *Trochoceras Halli*.

By far the greater part of the volume is occupied by the *Nautilidæ*, the following genera and subgenera being treated of:—

Trocholites, Conrad.
Gyroceras, de Koninck.
  Subgenus *Aipoceras*, Hyatt.
  Subgenus *Trigonoceras*, M'Coy.
Hercoceras, Barrande.
Barrandeoceras, Hyatt.
Discites, M'Coy.
  Subgenus *Phacoceras*, Hyatt.
Ephippioceras, Hyatt.
Cœlonautilus, Foord.
Pleuronautilus, Mojsisovics.
Temnocheilus, M'Coy.
  Subgenus *Centroceras*, Hyatt.
Solenocheilus, Meek and Worthen, emend. Hyatt.
Nautilus, Breyn.
  Subgenus *Hercoglossa*, Conrad, emend. Meek.
  Subgenus *Clydonautilus*, Mojsisovics.
Aturia, Bronn.

No new species is added to the genus *Trocholites*, but a new species of *Gyroceras*, "closely allied to *Gyroceras ornatissimum*, de Koninck, sp.," is described from the Carboniferous Limestone of Ireland under the name *Gyroceras hibernicum*, and upon a single specimen from the same horizon and locality is founded a new species of the subgenus *Aipoceras*, viz. *A. compressum*.

Prof. Hyatt included in Barrande's genus *Hercoceras* such forms as *Gyroceras alatum*, Barr., and *Trochoceras flexum*, Barr., in which the apertures are not contracted, but Mr. Foord restricts the genus to species which have the aperture peculiarly contracted, as in *Hercoceras mirum*, Barr., thereby retaining *H. mirum* (with its variety *irregulare*) as the only known species of the genus.

In addition to the two well-known British species, which fall into the genus *Ephippioceras*, viz. *Nautilus clitellarius*, J. de C. Sowerby, from the Coal Measures of Shropshire and Worcestershire, and *Nautilus bilobatus*, J. Sowerby, from the Calciferous Sandstone of Closeburn, Dumfriesshire, the author describes under the name *costatum* a new species, which occurs in association with *N. clitellarius*.

The name *Cœlonautilus* is comparatively new. The first mention of it seems to be in an exceedingly interesting paper which appeared in the Geological Magazine for November, 1889 (p. 494), by A. H. Foord and G. C. Crick, "On the Shell-muscles of *Cœlonautilus cariniferus*, J. de C. Sowerby, sp.," in a footnote to which paper it is stated that "this name from κοῖλον, hollow (referring to the

umbilicus), and *Nautilus*, is proposed by one of us in substitution for *Trematodiscus*, Meek and Worthen, which was used by Häckel for a genus of Radiolarians. The name *Trematoceras* proposed by Hyatt (Proc. Boston Soc. Nat. Hist. 1883, vol. xxii. footnote, p. 291) in lieu of *Trematodiscus* is equally ineligible, because preoccupied, for although the species described by Eichwald (Leth. Rossica, 1860, vol. i. p. 1259)—*Trematoceras discors*—was a *Bactrites*, a generic name once published cannot be again employed, even for a different group, without risk of confusion." Prof. Hyatt retains for the shells comprised in the present group the name *Vestinautilus* proposed by Ryckholt; but de Koninck pointed out that this name was evidently founded upon an error of observation, and since it "cannot be used in the sense intended by its author," Mr. Foord agrees with de Koninck in treating Ryckholt's name as a synonym. Mr. Foord reproduces the figures of the impression of the shell-muscles of *Cœlonautilus*, which appeared in the GEOLOGICAL MAGAZINE for November, 1889, and it is to be hoped that his examination of the large number of specimens of Ammonoidea contained in the National Collection will enable him to demonstrate also the form of the muscular impression in some of the Ammonites. Several new species of *Cœlonautilus* are described, as well as of the genera *Temnocheilus* and *Solenocheilus*.

Then follows the genus *Nautilus*, of which, according to a table given by the author in his Introduction, the Museum contains 88 species, viz. 5 Triassic, 30 Jurassic, 40 Cretaceous, and 13 Tertiary. Mr. Foord remarks: "It might perhaps have been supposed that a genus so rich in species as *Nautilus* is would have supplied many subdivisions or groups of species; but after carefully considering the matter, I have come to the conclusion that such groups are in the present case unnecessary, first, because the relationship in which the species stand to each other is fully set forth in the remarks appended to the descriptions of the species, and, secondly, because such groups are apt to become very artificial, owing to the necessity for frequent change in the selection of the characters upon which they rest. It is true that there are often met with in the genus *Nautilus* assemblages of species having many characters in common, which are, however, too variable to found genera upon."

Many of the species of *Nautilus* are comparatively new, having been described by the author and Mr. G. C. Crick in the Annals and Magazine of Natural History for April and May, 1890; some are quite new, but the excellent figures which accompany the descriptions of these species will greatly facilitate their identification.

It is to be regretted that the whereabouts of some of Sowerby's type-specimens is unknown. This is the case with *Nautilus simplex*. On comparing the characters given by Sowerby with Mr. Foord's description of his new species *semiundatus*, we think the author has good grounds for suggesting the possibility of Sowerby's specimen being a badly-preserved example of this new species. He adds (p. 285), "Unfortunately, the siphuncle has not been seen in *N. semiundatus*, and therefore it cannot be invoked in aid of this com-

parison." Doubtless he meant to have said that the siphuncle has not been seen in the adult stage, for in his description of *semiundatus* (p. 280) he observes, "The siphuncle in a young individual (No. 36603), $1\frac{3}{4}$ inches in length, is very near the inner (dorsal) margin of the septa." The position of the siphuncle then, so far as it is known, constitutes another point of agreement with Sowerby's *N. simplex*.

As the author has not been able with abundant material before him to determine to what maturer form Sowerby's small specimen of *N. inæqualis* belongs, we quite agree with him that it is much better to drop the name altogether. The specific determination of very young Nautili is often absolutely impossible.

Passing on to the genus *Aturia* we notice, that while Edwards in his Monograph of the Eocene Mollusca (Pal. Soc. 1849) described and figured two varieties of the well-known *Aturia ziczac*, Mr. Foord reserves this name for the form figured by J. Sowerby in plate i. of his Mineral Conchology, and gives a new name, *A. Charlesworthi*, to the more compressed shell which Edwards distinguished as var. β. The latter comes very near to the Dax shells *Aturia Aturi*, Basterot, but the differences in the suture-line and the amount of compression are considered sufficient to justify the separation of the species. As to the *Nautilus lingulatus*, v. Buch, from the Eocene of Kressenberg, Bavaria, Mr. Foord concurs with Dr. Geinitz in referring it to *Aturia ziczac*, J. Sowerby, sp.

The concluding section on the *Nautilidæ* deals with the Rhyncholites or the mandibles of fossil Nautiloids. Various names have been given to these fossils, and various opinions have been entertained as to the advisability of referring them to genera and species. Although it has been suggested that Rhyncholites may be of more importance for the purpose of classification than the guards of Belemnites, Mr. Foord refrains from referring them to genera and species, (1) owing "to the want of stable characters upon which to found genera and species," and (2) because the material is not "sufficiently abundant to enable one to trace the variations that may arise in different stages of growth, and thus to afford data for the limitation of such species as may be constituted," and he adds, "Under these circumstances I have merely figured some of the principal types of the fossil beaks met with in the Jurassic and Cretaceous rocks of England which, so far as I am aware, have not been illustrated." The figures are exceedingly good, and in addition to the fossil beaks the author gives some admirable figures of the upper and lower mandibles of the recent Nautilus (*Nautilus pompilius*).

Dr. K. A. von Zittel, writing in 1884 (Handb. d. Palæont. Band II. p. 386), states that hitherto lower mandibles have been found only in the Muschelkalk. Mr. Foord's examination of the National Collection has, however, enabled him to record and figure specimens from the Lias of Lyme Regis and the Lower Chalk of Dover.

The Rhyncholites are so rarely associated with the Cephalopod to which they belong, that the specimens of *Nautilus Libanoticus*

in the Museum that the author states have one or other of the mandibles *in sitû* must be regarded as treasures indeed. A figure of one of these specimens is given, showing the mandible upon the surface of the cast of the ventral aspect of the body-chamber.

The figures in the book are both numerous and excellent, and we feel quite sure that the volume will prove a most valuable addition to our knowledge of the Cephalopoda.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—May 27, 1891.—Sir A. Geikie, F.R.S., President, in the Chair.—The following communications were read :—

The Secretary announced the presentation by Sir John W. Dawson, LL.D., F.R.S., of a series of photographs of Nova-Scotian Coal-Measure Amphibia and Reptilia, and read an explanatory note written by the donor.

Dr. G. J. Hinde remarked that additional interest attached to the genus *Hylonomus* from the fact that a representative of it had lately been discovered in the Burnley Coalfield, and described by Mr. A. Smith Woodward, F.G.S., in the May Number of the Geological Magazine under the name of *Hylonomus Wildi*.

1. "On the Lower Jaws of *Procoptodon*." By R. Lydekker, Esq., B.A., F.G.S.

After reviewing Sir R. Owen's writings upon the large extinct Kangaroos for which he established the genus *Procoptodon* in 1874, the author describes two mandibular rami from the clay beds of Miall Creek in the neighbourhood of Bingera, N.S.W., which belong to this genus, and from their characters and a comparison of them with the lower jaws in the British Museum, he maintains that this part of the skull indicates two very distinct species of the genus, for which he retains the names *P. rapha*, Ow., and *P. goliah*, Ow., though it is possible that the types of those two species are really specifically identical, in which case the name *P. pusio*, Ow., might have to be adopted for one of the species described.

2. "On some recently exposed Sections in the Glacial Deposits at Hendon." By Henry Hicks, M.D., F.R.S., Sec. Geol. Soc.

In this paper the author brings forward evidence obtained from sections exposed in gravel-pits and deep cuttings made for the purpose of laying down the main sewers, to show that Glacial Deposits had been spread out to a much wider extent over the Hendon plateau than had hitherto been supposed, and that they had reached down the slopes to below the ordnance-datum line of 2000 feet. He further mentions that there is evidence to show that these deposits have extended in a S. and S.W. direction across the Brent and Silk Valleys, and now occur on most of the heights in the parishes of Kingsbury and Willesden. As the sands, gravels, and Boulder-clay which cover the Hendon plateau and the neighbouring heights are found to rest on an undulating floor of London Clay, and to follow

the contours of the hills and valleys, the author considers that it is clear that the main physical features of this portion of N.W. Middlesex were moulded at a very early stage in the Glacial period, and before the so-called Middle sands and gravels and overlying Upper Boulder-clay with Northern erratics, were deposited. He believes that at this time there could have been no barrier of any importance to prevent these deposits from extending into the Thames Valley, and that the evidence clearly points to the conclusion that the implement-bearing deposits on the higher horizons in the Thames Valley should be classed as of contemporaneous age with the undoubted glacial deposits at Hendon, Finchley, and on the slopes of the Brent Valley, which they so closely resemble. The author is therefore satisfied that man lived in the neighbourhood of the Thames Valley in the early part of the Glacial period; probably, he thinks, in pre-Glacial times.

CORRESPONDENCE.

CORRELATION OF QUATERNARY CHANGES OF LEVELS IN NORTH AMERICA AND THE CARIBBEAN REGION.

Sir,—Referring to Mr. Jukes-Browne's letter in this Magazine for March (p. 143), and to his and my preceding articles and correspondence therein cited, I have to reply that the questions which he asks are manifestly very difficult; but certain points may be noted which partially answer them. If the end of the Tertiary era and beginning of the Quaternary were characterized by elevation of the greater part of the North American continent to such altitude as to give a much cooler climate, and by a contemporaneous depression of the West Indies and the Isthmus of Panama, allowing a large part of the equatorial Atlantic current to pass into the Pacific Ocean, the result of these combined conditions being the accumulation of the ice-sheets of North America and Europe, we should expect to find west of the Gulf of Mexico, as Mr. Jukes-Browne suggests, evidences of recent changes of levels. The subsequent depression of the continent north of the Gulf and the uplift of the West Indies and the Panama region, bringing the present relative heights of land and sea, may have produced the east to west line of fissuring and faulting which crosses Mexico near 19° north latitude, shown by the very remarkable series of volcanoes of Tuxtla, Orizaba, Popocatepetl, Ixtaccihuatl, Toluca, Jorullo, and Colima. Farther west, this line of disturbance in the earth's crust probably extends to the volcanic Revillagigedo Islands. Eastward, after crossing the base of Yucatan, it appears to be represented by the Great and Little Cayman Islands, and by the Sierra Maestra on the south shore of eastern Cuba, with the contiguous "Bartlett Deep," a very profound narrow trough of the Caribbean Sea, reaching from Honduras Bay to the Windward Passage; and thence the same orographic belt is continued by Santo Domingo, Porto Rico, and the Windward Islands, with the very deep oceanic troughs north, south, and south-east of Porto Rico (bathymetric map of the Caribbean region, A. Agassiz, Three Cruises of the "Blake," vol. i. fig. 57).

The disturbances along this belt, however, have been of a different order from the uplifts and subsidences which have affected the whole continental area northward from the Gulf of Mexico, as known by the Pleistocene submergence of river valleys and fjords on the Atlantic, Pacific, and Arctic shores of North America. No less than twenty submerged valleys, some of them extending to depths of 2000 to 3000 feet, have been found by soundings on the coast of California by Prof. George Davidson, of the U.S. Coast Survey; and Prof. Joseph Le Conte has shown that the time of elevation during which they were eroded was the Pliocene and early Quaternary, which also included the plication of the Coast Range, the outpouring of the lavas forming the Cascade Range, and the faulting and tilting that elevated the Sierra Nevada, Wahsatch, and Basin Ranges to their present height (Elements of Geology, new ed. 1891; Bulletin G.S.A. vol. ii. 1891, pp. 323–330). On the Atlantic coast, too, the submerged valleys mentioned in previous articles were doubtless eroded during the same time. Excellent evidence of this is given by the submarine channel of the Hudson, partly a profound fjord, extending about 100 miles beneath the sea, and descending to the depth of 2844 feet; for samples of its bottom and banks, brought up by the sounding-lead, appear to belong to a continuation of the Tertiary sandy clays of New Jersey (Am. Jour. Sci. III. vol. xxix. pp. 475–480, June, 1885; vol. xl. pp. 425–437, Dec. 1890, with map). These deeply submerged, narrow river channels, and the similar Arctic fjords, together bear testimony of a late Tertiary and early Quaternary elevation of nearly all of North America to such altitude that the resulting colder climate would induce glaciation. So widely extended continental uplifting, and the later Pleistocene depression of the same area, belong to a class of the earth's crustal movements which Gilbert and White have called epirogenic, that is, continent-making, in contrast with orogenic or mountain-making upheaval and subsidence.

In the Caribbean region depressions succeeded by elevation during the Quaternary era, which are known from the raised coral reefs studied by Mr. Jukes-Browne and others, belong to orogenic movements, chiefly by faulting, uplifting, and tilting, which have taken place on a most grand scale in this era throughout the chain of the West Indies and Windward Islands, and along the entire Cordilleran belt from Cape Horn to Panama, to the Sierra Nevada and Great Basin, and to Mount St. Elias and the Aleutian Islands. These mountain-making disturbances have been closely related with the great epirogenic uplift and depression of North America, and with their duplication in two distinct Glacial epochs, divided by a very long interglacial epoch on this continent; but to attempt discussion of details of their correlation, or to speculate upon the condition of the earth's crust and interior permitting such changes of levels, would require too much space for a letter. Some notice of these matters will be found, respectively, in my article in the American Journal of Science for January, 1891, and in my appendix of Wright's "Ice Age in North America."

SOMERVILLE, MASS., *April 6th*, 1891. WARREN UPHAM.

A VARIETY OF PICRITE (SCYELITE) IN SARK.

SIR,—Rather more than two years since (Dec. III. Vol. VI. p. 109) I wrote a description of a variety of Picrite which I had found in boulders at Port du Moulin, Sark, stating that I published it, as there was "no probability of my returning to Sark for years, if ever," in the hope some one would trace this interesting rock to its home. But the unexpected often happens: last summer's work among the hornblende schists of the Lizard determined Mr. Hill and myself to re-examine those of Sark, and in the process of this the picrite was not forgotten. After a careful search along the rocks at low water we found a dyke of this rock at the foot of the cliffs between Port du Moulin and Saignie Bay, nearly opposite to (perhaps rather to the south of) the Grand Autelet. It is at the base of a little spur from the cliff of banded gneiss, into which it is intrusive, but it only shows for a foot or so above the shingle, in two or three humps, running seawards. In this direction, about fifteen yards off among the boulders, is another boss. I have examined the rock under the microscope. The olivine is not so well preserved, there is rather less mica and more hornblende than in the specimen described in 1889; it is not quite so obviously connected with the serpentines, but I have no doubt that the boulder came from some part of this dyke. We searched the cliffs very carefully up to the further side of Saignie Bay on the north, and for some distance to the south of Port du Moulin without finding any other dyke. We now think it very improbable that Ansted, in speaking of a dyke of serpentine as crossing the island, referred to this rock. We reserve further particulars for a paper in which we hope to communicate to the Geological Society the result of our work in Sark.

T. G. BONNEY.

OBITUARY.

PETER MARTIN DUNCAN,

M.B. (LOND.), F.R.S., F.G.S., F.L.S., ETC.

BORN, 20TH APRIL, 1824. DIED, 28TH MAY, 1891.

PETER MARTIN DUNCAN was born at Twickenham in 1824, and received most of his early education in the Grammar School there. After leaving this he lived for a short time in a school in Switzerland, and on his return to England entered the Medical Department of King's College, in September, 1842. He there received the whole of his formal scientific training; he passed the preliminary M.B. examination with honours in Anatomy and Physiology in 1844, and obtained the full degree in 1846; he was elected an Associate of his College in 1849. Upon the conclusion of his medical studies, he acted for a time as assistant to a doctor at Rochester, whence he removed to Colchester, where a practice had been purchased for him. Here he remained for many years, and it was during this period that he published his first scientific essay, which consisted of "Observations on the Pollen tube, its growth, histology and physiology" (1856). But he did not at Colchester secure much time for original research, for most of that which was left him by his profession was occupied

by work in connexion with the municipal politics of the borough, in which he seems to have played a prominent part. The fact that he served as Mayor shows that he had won the confidence of his fellow-townsmen, while the admirable arrangement of the local Museum, which under his direction was reorganized upon lines far in advance of the time, is a sign of his interest in the educational institutions of the town. After his return to London to a practice at Blackheath, he was able to spare more time for scientific work, specializing upon the Corals; and as his interest deepened in the problems which these presented him, he was led to abandon lucrative prospects in his profession and devote himself entirely to original research. In this he was no doubt encouraged by the reception accorded to his first palæontological papers, which were read in 1863; they at once gained him recognition as one of the ablest of British palæontologists; he was in the following year appointed one of the Honorary Secretaries of the Geological Society, and two years later he was elected F.R.S.

After leaving Blackheath he settled near Regent's Park, but he was not long allowed to remain in retirement, as in 1870 he was called to the chair of Geology at King's College, and a Fellowship followed in the next year. Shortly afterwards he accepted also the Professorship of Geology at Cooper's Hill, and he held both appointments till his death. He resigned the Secretaryship of the Geological Society in 1870 after a seven years' tenure of office through a period in which the change of apartments had made the duties more than usually onerous. In 1872 he was elected one of the Vice-Presidents, an office which he held till his promotion to the Presidency in 1876 and 1877. In 1881 he was awarded the Wollaston Medal, the highest honour which the Geological Society can bestow. Though it was the Geological Society with which he was most closely connected, he was an influential member of other scientific bodies; he served on the Council of the Royal Society from 1876 to 1878, was President of the Geological Section of the British Association in 1879, and of the Microscopical Society from 1881 till 1883. By his resignation of this post he terminated an official career of no ordinary usefulness, and retired to Gunnersbury, where he passed the remaining years of his life.

On turning to Prof. Duncan's scientific work, one is impressed by the enormous amount he accomplished and the wide range of his interests and influence. As has been previously remarked, his first paper (1856) was botanical, and he long retained his interest in the subject, his last paper on vegetable physiology being published in 1874; while still later he worked out the parasitic algæ which he discovered in some of his Silurian Corals. His first important work was the series of five memoirs on the Fossil Corals of the West Indies, a subject which he took up, as at that time he failed to get the necessary facilities for the study of the recent forms. The subject was full of difficulty; the living Corals of the area were but little known, so that the materials for the comparison of the recent and fossil faunas were quite insufficient. But Prof. Duncan attacked the subject with characteristic energy, and his sound

common sense enabled him to avoid many a pitfall; his memoir was certainly a most valuable addition to the knowledge of the later Tertiary Corals. This work was followed by a long list of papers and monographs in which he described the Coral faunas (especially the Cainozoic) of England, Australia, Tasmania, India, Java, Arabia, and Malta. His "British Fossil Corals" is probably one of the finest contributions to English Palæontology ever published by the Palæontographical Society; it was so much more modern in its methods and more thorough in its treatment than the less painstaking work to which it was issued as a Supplement.

But though Prof. Duncan's interests were probably at first rather zoological than geological, he soon became absorbed in the line of work which he had been led by circumstances to select. He early realized that the description of the anatomical structure and the determination of the systematic position of a fossil did not constitute the sole duties of a palæontologist. With him these were but preliminary to the consideration of the affinities of faunas and their bearing on the physical geography of the past. He was a palæontologist in the truest sense of the word,—not a morphologist who happened to study extinct forms, but a geologist who used fossils as a petrologist uses minerals. Hence his early work on the West Indian Corals commenced by a detailed study of their conditions of fossilization and closed by a discussion of their evidence as to the Cainozoic physiography of the Caribbean region; similarly his later studies of the European Corals led to his striking paper on "The Physical Geography of Western Europe during the Mesozoic and Cainozoic periods elucidated by their Coral Faunas."

It was probably his desire to check the conclusions yielded by the Corals that led him to take up also the study of the Echinoidea, and as work with these is more definite than with the former, it yielded him some of his most interesting conclusions. He commenced with the Echinoids of beds, the Corals of which he had already studied; among the most remarkable were the collections from South Australia, which he described in a series of papers dating from 1864 to 1887. It was apparently his interest in the origin of this fauna, with its mixture of Cretaceous and Cainozoic genera, that led him to turn with such zest to the Indian Echinoids, which, in conjunction with Mr. Sladen, he monographed with great detail and care.

Of the Mesozoic Echinoidea he studied with especial interest those of the Cenomanian, and by the aid of the small collections of the Rev. W. F. Holland, in Sinai, and Dr. Carter, in South Arabia, he gradually built up the connexion of the European fauna with that of Northern India. And then, by his comparison of those of the Peninsular and Extra-Peninsular areas, he demonstrated the existence of the land barrier that stretched across India and away to the south-west, of which such important use has been made in recent controversy. His views on geographical distribution were original and had been carefully matured; his lecture on "The Formation of the Main Land Masses" showed that he did not accept the view of the

permanence of oceans and continents, a subject upon which his opinion was of especial value. Another of his contributions to chorology was his paper on the fauna of the Alpine Lakes, perhaps the most serious blow ever struck at the theory of the Glacial origin of the Swiss lake-basins.

But though Professor Duncan did not regard morphology as the highest end, he did not by any means neglect it; thus our knowledge of the perignathic girdle of the Echinoids and its value in classification we owe mainly to him; while his remarkably suggestive and original essay on the structure of the ambulacra of the regular Echinoidea, perhaps his most masterly piece of work, has gained the highest praise from men prejudiced against him.

But in addition to his contributions to palæontology he has done much in zoology: he wrote a series of papers on the anatomy of the Temnopleuridæ, Saleniidæ, and other groups of the Echinoidea, and described, amongst others, the Madreporaria of the *Porcupine* Expedition, the Ophiuroids and Corals of Mergui, and in conjunction with his constant collaborator, Mr. Sladen, the Echinodermata from Greenland. It was probably his close study of the recent forms that made his judgment usually so sound, while the knowledge gained was indispensable for the preparation of his two invaluable works "The Revision of the Madreporaria" and his "Revision of the Genera and Great Groups of the Echinoidea." The former was issued in 1885, and consisted of diagnoses of every genus of Corals (excluding Rugosa) and of a classification which has not yet been supplanted. His Revision of the Echinoidea was perhaps still finer, and made a great advance in our knowledge of every order. The application of his own discoveries on the ambulacral structure enabled him to bring the Palechinoidea from chaos into order, and to replace the artificial arrangement of the Diadematidæ by a natural classification; his previous detection of the fundamental differences between the pits of *Temnopleurus* and the fossettes of *Temnechinus* gave him the clue to the arrangement of that group; and his substitution of positive for comparative diagnoses in many recent genera has greatly aided the comparison of the fossil and deep-sea types. But his use of the perignathic girdles in another order was less successful, while his acceptance of Lovén's results brought him into conflict with some continental palæontologists regarding the classification of the Spatangoidea. By these two Revisions alone Prof. Duncan has earned the gratitude of every palæontologist and zoologist; they precisely formulated the best current thought of their time, and have given a firm basis for future work. They must be indispensable works of reference to every student of these groups, till in years to come the progress which they have so largely aided has rendered necessary a new revision, and until some one is then ready and able to undertake the enormous labour such a task involves.

But in addition to the Corals and Echinodermata, Prof. Duncan made some contributions to the study of the Protozoa and Sponges, while his clearness as a teacher led him to undertake a good deal